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THE GERMINAL BACKGROUND OF SOMATIC MODIFICATIONS[1]

By Dr. M. F. GUYER

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ON the theory apparently that all pleasures must carry a penalty of compensatory pain, some former grim group of zoological Puritans decreed that though this organization may eat, drink and be merry, it must in the end endure a retiring address, so-called presumably because it conduces to the usual result of retiring, sleep. No vice-president of Section F is ever brave enough to break the bonds of this ancestor worship, so you and I, fellow zoologists, are here alike victims of a tradition. Let's be merciful!

In venturing to speak briefly on the germinal background of somatic acquirements, you will observe that I have reversed the traditional order of this combination, the germinal foundation preceding the acquirement. Possibly this is the only novelty you will find in my remarks. But since general topsyturviness is the order of the day, I can at least plead modernity. In these days when children run their parents, freshmen instruct their professors and wives support their husbands, why should not some of our venerable biological riddles be approached backward?

We used to dream of romance, with each yearning soul finding its unerring way to its predestined mate; we now talk of propinquity and endocrines. The dart of Dan Cupid seems slated for replacement by the hypodermic needle and a proper blend of hormones. We used to hear of sin; we now know only psychoanalysis and unsatisfied self-expression. We used to read of prepotencies and pointings, but this lore of the fancier has given place to the chemistry of the gene. So in such precedents of revolution and dis-

[1] Address of the vice-president of Section F—Zoological Sciences, American Association for the Advancement of Science, December 31, 1929.

illusionment I would again justify my means of approach to a very old problem.

The possibility of modifying germ-plasm stands as a perpetual challenge to the biologist because the problem of variability, of the origin of new characters, constitutes the very foundation of both genetics and evolution. Until we know more of the causes of germinal variation than we do to-day, we must remain ignorant of the most essential factor in each of these fields. So it is obvious why, although baffled at almost every turn, the experimenter returns again and again to his attack on the central problem.

Great skepticism prevails among geneticists about interventions of the parental body affecting the germ-cells, particularly in any specific way. Since on the basis of negative evidence at least, the efforts of the flesh seem unavailing in any perceptible modification of germ-plasm, the modern student of heredity almost universally falls back upon the non-committal conception of a directly varying germinal substance, and simply pleads ignorance of the causative stimuli, internal or external, that induce such changes.

The truth is that in the very setting of the problem of the inheritance of somatic acquirements, we are, from one point of view, faced by an apparently insoluble dilemma. For the very fact that a character can be acquired by the body carries with it the inescapable admission that the constitutional capacity for its appearance already exists. It is a commonplace of modern genetics that the characteristics of an organism require for their expression not only an adequate stock of germinal materials which we call genes, but a suitable environment. From this point of view, an acquired character is one in which not the *genic constitution* but the *environment* has been changed.

Such a statement of the problem, however, meets the immediate challenge of explaining how such an acquired character is related to the genes of the germ, but inasmuch as we do not know how any character, acquired or otherwise, is related to the germ, our ignorance is no more dense than it was before. Certainly if our modern knowledge of genetics teaches us anything, it is how little we know about the relationship that exists between the chemical complexes of the germ and the expressed characteristics of the body. We know that any so-called character of the body is the indirect, cumulative product of a long series of interactions of these original chemicals; that great numbers of genes must cooperate to produce even the simplest character; that no one of these genes more than another represents the character in any literal sense, and that any particular gene influences not only one but many characteristics of the body.

There is an increasing amount of experimentation, particularly with X-ray and with radium, which shows that external influences may directly affect germ-plasm and that the resulting changes may become hereditary. The brilliant successes of Muller[2] and his associates in this field through radiation of Drosophila are of so recent occurrence as to be fresh in the minds of you all. Muller, you will recall, by means of X-ray bombardments increased the frequency of mutation about 150 times. Many of the induced mutations were the same as had already been found to occur in ordinary cultures of Drosophila; many were new. As with all observed mutations, including the four hundred odd which have been discovered in Drosophila alone, most were detrimental to the organism. Likewise most of them were recessive. The results seemed to be the random effects of ultramicroscopic bombardments, since even the twin gene of the closely associated homologous chromosome remained unaffected. Since the gene must be a chemical as well as a genetical entity there seems no reason to doubt that the outcome is due to a permanent change in its molecular constitution. As Muller graphically puts it: "The roots of life—the genes—had indeed been struck, and had yielded." He points out, furthermore, the similarity of the X-ray mutations to natural mutations which have been found in Drosophila and concludes that "the minute amounts of natural radiation present almost everywhere in nature . . . some of it of terrestrial origin, derived from the radium and other radioactive substances in earth, water and air, and a smaller part of it of cosmic origin, apparently derived from the diffuse and distant factories of matter . . . all this natural radiation *must* be producing some mutations in the living things on earth."

Within just the past few months Babcock and Collins have announced actual experimental demonstration that short-wave radiations from the earth itself either cause mutation or affect its rate of occurrence. They exposed genetically similar strains of fruit-flies in two different localities, one the university campus at Berkeley, California, the other in Twin Peaks tunnel, San Francisco, where the rock gives off about twice as much radiation as does the campus soil. After five months of culturing in these respective localities a check-up of their records showed about twice as much mutation in the tunnel cultures as occurred in the campus controls. The mutation studied was a "sex-linked lethal" which caused all males to die before hatching, leaving only females in the new generation. The differential percentage in the two localities was found to be constant and consistent. And at the present meeting we have heard Dr. Hanson, although working entirely independently

[2] *Scientific Monthly*, December, 1929, p. 481.

and in a different region, report essentially similar results.

Thus, we can scarcely overestimate the importance of Muller's findings and of the corroborations, extensions and new applications by such workers as Weinstein, Hanson, Dobzhansky, Stadler, Whiting, Blakeslee, Buchholtz, Patterson, Harris, Oliver, Gowen, Babcock, Collins and others, for here at last we have a plausible escape from that hoary fiction, a "spontaneous variation." Whether or not we have the exclusive cause of gene mutations remains to be seen.

To many of us the outstanding perplexity in the field of variation is not so much the mere appearance of changes in the germinal substance as the appearance of the particular kinds of change that eventually lead to that hand-in-glove relationship between organism and environment that we call adaptation. The accomplished facts seem almost overwhelmingly to suggest that environment must play an important part in initiating as well as in conditioning particular kinds of germinal changes.

Time and again in the past, according to paleontologists, whenever conditions arose that would permit of the existence of new types of living organisms, forms admirably adapted to those conditions appeared. In some way the environment molded these new inhabitants to its bounds, and it seems well-nigh incredible—no matter what eons of time are vouchsafed us—to believe that this has been done merely by the negative method of killing off generation after generation of the non-conformists. For on such a basis not only has the adapted organism had to await the accidental occurrence of a favorable germinal variation, but of hosts of them, often highly interrelated, which then must be sifted and perfected by natural selection through innumerable generations to bring about that marvelous fitness which characterizes living things.

Muller himself[3] gives us an interesting argument favoring the view that, together with natural selection, the multiplicative power of mutated individuals, particularly the favored few as against those having disadvantageous or neutral changes, is all that is required for "turning accident into order," and he contends that natural selection need operate with barely enough stringency to permit the multiplication of that rare minority in a generation in which the new mutations have chanced to be favorable.

Not only the facts of intricate adaptation, however, but various facts of geographical distribution likewise incline one to suspect that altered function or environment, if long continued, is directly instrumental in molding the fauna of a given region; yet experimental proof of such direct influence is lacking. Particularly when one contemplates the highly specialized adaptations of many parasites does he feel skeptical of the doctrine of an all-sufficient natural selection based wholly on accidental variations. For the adaptive mechanisms or adjustments of many of these parasites, whether such internal forms as intestinal protozoa or such external ones as bird-lice, must be of comparatively recent origin. Since they are often so highly specific that they can not live on even a slightly different species of host, it is evident that they could have reached their own state of highly specialized adaptation only after the host itself had evolved into a distinct species.

As just the reverse of what appear to be more rapidly or recently adapted forms, we have, on the other hand, our so-called "persistent types" which have remained in sluggish apathy for countless eons while their more versatile brethren have run a wide gamut of evolutionary change. One wonders how their genes escaped this radiant bombardment which set so many of their contemporaries on a course of further adaptation; or if their environment put no premium on change, why they did not at least enter upon a course of non-adaptive evolution through the accumulation of chance hit-effects.

Then again we have to face the problem of what appear to be definite trends in evolution as evidenced by similar changes which have appeared in great groups of separate though allied genera and species of animals since their respective departures from the common ancestral group. I refer to what is sometimes termed orthogenesis, though I hesitate to use this term because it means so many different things apparently to so many different people, ranging all the way from some mystical inner perfecting principle, to merely a general trend in evolutionary development due to the natural constitutional restrictions of the germinal materials, or to the physical limitations imposed by a narrow environment. The impression is held by many students of evolution that certain variations are prone to occur more frequently and more widely than others, in various related organisms, and some believe that such changes, irrespective of whether they are helpful or harmful to the species, tend to accumulate in definite directions. If favorable, it is obvious that their perpetuation is more assured. Since, in a former paper, I have commented rather fully on various lines of this evidence, I shall not review them here. It is sufficient for present purpose to say that many investigators have pointed out parallelisms in variation which tend to appear in different branches of the same large group of organisms.

One of the most recent studies of the kind with which I am acquainted is that of Metcalf[4] called

[3] *Loc. cit.*

[4] *Jour. Morph. and Physiol.* 45: 1, March, 1928.

"Trend in Evolution." In the Salpidae, for example, he finds the same trends in certain characteristics of gut, muscle and eyes appearing in both the solitary and the chain salps. These modifications are most pronounced in the phylogenetically highest groups. While in the colonial types the changes at their inception are probably adaptive or at least not disadvantageous, to the solitary salps they are harmful. Metcalf believes, furthermore, that certain of these evolutionary changes, even though not utilitarian, have been thrown back in the germ-plasm in some way so that they appear far earlier in the individual life cycle than their late phylogenetic origin warrants. Again, he is so impressed with the irregular occurrence of similar characters in subdivisions of such groups as the Opalinidae "along the stem and branches of the phylogenetic tree," as he phrases it, that he sees no reasonable alternative to the conception that the evolution of the family is largely a working out of tendencies inherent in the germ-plasm.

Many other examples of parallelism in related forms will come to the mind of any one who has made taxonomic studies. To take an example from the vertebrates, the same features of color and color-pattern, such as the formation of eye-spots, barring and the like, appear again and again in various genera and species of the pheasant subfamily (Phasianinae). If the original mutations were fortuitous they seem to have instituted a series of changes in definite directions. To be more specific let us glance briefly at the varying stages and types of ocellation to be seen in the interesting group of peacock pheasants (Polyplectron) regarded by systematists as intermediate between the peafowls and the pheasants in the narrower sense. In *Polyplectron chalcurus,* apparently the most generalized species, ocellation does not occur. The only hint of what is to be realized in the more specialized members of the group is found in a pronounced purplish and greenish metallic coloration on certain feathers of the tail. In the male of *P. emphanes,* while there are numerous green metallic iridescent areas on the feathers of the upper wings and back, they have not yet progressed to the condition of being definite ocelli, although on the tail of this same individual there are two transverse bands of ocelli. As a further extension, in the male of *P. thibetanum,* the small feathers of the wings and the feathers of the interscapular region bear distinct small purple ocelli ringed successively with black, light brown and white. The tail is also banded with ocelli. In the male of *P. germaini* the wing-coverts and back bear numerous green ocelli. In the common peacock, which belongs to a related genus, the conspicuous "eye-spots" on the tail are, of course, known to all. Thus this tendency toward the formation of eye-spots in various genera and species of pheasants, apparently hinted at even in the greenish-black iridescence so often visible in the tail-feathers of the common rooster, would seem to be the outcome of a germinal bias which finds different ranges of expression in different species. In some it occurs as eye-spots on the wing-feathers, in others, on the body-feathers; in one species there may be a single, in another two rows of ocelli across the tail. Since these patterns appear in collateral lines, it is obvious that they have not been derived directly one from another but are the outcome of a general tendency present in the group as a whole. One sees in its incipiency in one species a character which may have reached an advanced expression in a kindred group, or finds various more or less intermediate expressions in other related species. The very fact that, instead of existing as a medley of wholly unrelated elements, certain characteristics such as color markings can frequently be arranged as parts of a definite pattern or as stages in a general process indicates directional variation. If one chooses the features of bars and spots and their intergradations in the guinea subfamily (Numidinae) the story is much the same.

The paleontological record also affords many examples of what appear to be directed series of change, even in some cases seemingly to an overdevelopment of structure or to physiological disfunction which has meant final extinction of the species. In such cases possibly for formal explanation we might fall back upon the assumption of some influence of modifying genes or of different equilibria among modifying genes, but then we should have also the problem of accounting for the modifying genes and how they come to operate in seemingly so orderly and determinate a way.

With facts, or what appear to be facts, such as these facing us, then, without denying the probability of fortuitous gene mutations and their initiation by radiant emanations, there yet remains the possibility that mutations may also be otherwise engendered, or that once a gene mutation occurs it becomes more or less subject to other forces within the germ-plasm. Surely genes must be nourished like other living things, since they multiply and grow and apparently display the other characteristics of living matter. They must therefore be open to the vicissitudes which beset other living substance.

Is the gene itself, or that possibly larger vital unit of which it is a part, the fixed unvarying thing we tacitly regard it as being? May it not wax and wane in strength on occasion or may it not have primary and secondary parts like a molecule with side-chains, or be a polymeric particle which may have fewer or

more molecular units? Bits of evidence are occurring which suggest some such contingency.

As far back as 1917 Emerson[5] suggested "temporary inactivation" of the gene in certain cases of variegation, and in a similar connection Correns[6] in 1919 conceives of the gene as consisting of a large molecule with variable numbers of duplicate side-chains attached so that "to each number of these side-chains would correspond a definite ratio of white and green in the mosaic plant." This idea of a central molecule and side-chains has a very familiar sound to the speaker inasmuch as he discussed such a conception at some length before the American Society of Naturalists, at Ithaca in 1910.[7] Again, in 1924, in attempting to explain variegation, we find Eyster[8] postulating *genomeres,* gene elements which can segregate during development. According to him these may or may not be chemically identical. Likewise in 1927 Lillie[9] questions the view that genes are always present in the same quantity. Earlier, in 1916, Sewall Wright[10] speaks of "four quantitative gradations of one factor, which determines the amount of the basic color-producing enzyme."

Perhaps the most elaborate and consistently worked out theory of quantitative gene differences is that of Goldschmidt[11] who, in his theory of balanced action of the gene, discusses at much length the effects of the same gene in different quantities. He has been led to his present position through his attempts to analyze cases of intersexuality which he is able to initiate at will in the gipsy-moth.

Experiments and studies in sex determination and sex control all tend toward the conclusion that each sex in addition to its own determiners also contains the genes for the production of the other sex. The actual sex of the individual, leaving out the question of sex hormones and of other possible modifiers, is determined by the quantitative relations between these two sets of genes. According to Goldschmidt one of these sets resides within the sex chromosome, the other outside it. Under ordinary conditions two doses of the gene within the sex chromosome, or in other words, two sex chromosomes, produce the sex represented by the chromosome, but with only one sex chromosome, the outside determiners prevail. Where the male is heterozygous for sex as in Drosophila the sex chromosome carries the determiner for femaleness, and where the female is the sexual heterozygote, as in Abraxas, the sex chromosome bears the determiner of maleness.

[5] *Genetics,* 2, 1917.
[6] *Sitzungsber. Preuss. Ak. Wiss.,* 34, 1919.
[7] *Am. Nat.,* 45, May–June, 1911.
[8] *Genetics,* 9, 1924.
[9] SCIENCE, 66, 1927.
[10] Carn. Inst. Pub., 241, 1916.
[11] *Quart. Rev. Biol.,* September, 1928.

Goldschmidt succeeded in isolating different races of the gipsy-moth, of different sex-determining potencies; that is, different ones of them had different but typical quantities of the female- and male-producing genes respectively. By selecting his races Goldschmidt could get ones with quantities of maleness or of femaleness which, in the resulting cross, no longer balanced the quantities introduced from the other parent, with the result that, at will, any predictable degree of intersexuality up to and including sex reversal in either direction could be secured. In the gipsy-moth, since the female is heterozygous for sex, ordinarily two sex chromosomes indicate male, and one, female. However, by choosing a race with high quantities of the outside female-determining substance and crossing it with an individual in which the two sex chromosomes came from a race which possessed in these chromosomes a very small quantity of the male-producing gene, Goldschmidt secured what were actually females in spite of the fact that their gametic constitution (with two sex chromosomes) was that of males. He asserts, "If a given race is crossed in one direction with another test-race we can predict every result of crosses with all other races in every direction." And he goes on to say, "It is claimed as a fact that in our work on intersexuality different quantities of one gene have been studied."

Digressing from these special instances, Goldschmidt enlarges his conception to make it apply to all genes, drawing on such fields as multiple allelomorphism, irregular chromosome distribution and the like as the chief sources of his argument.

He conceives of the gene as operating through "the production of chains of reaction of definite velocities which are a function of the quantity of the gene in question." As to the nature of any given gene, he regards it as a "definite quantity of something (of course qualitatively different things in many different genes) linked with a chain of reactions with a velocity proportional *ceteris paribus* to its quantity." He believes with many others who have arrived at much the same conclusion that in the present state of our knowledge we can best regard it "as a type of enzyme and specifically as an autocatylist."

Again, such remarks about chains of reaction and catalysis have a familiar sound to the present speaker because in his 1910 paper[12] before the naturalists, already referred to (and even in an earlier paper,[13] 1907), he discussed at some length the probable functions of the chromosomes as the source of enzymes which initiate series of progressive reactions that result in chemical interactions. The following excerpts are as valid to-day as when they were written:

[12] *Am. Nat.,* 45, 1911.
[13] SCIENCE, June 28, 1907.

It is a well-known chemical fact, moreover, that when two or more progressive reactions are going on simultaneously, a quickening or retardation of the velocity of either, with the consequent precocious development of certain stages in the sequence, may lead to a partial or complete deflection of the original trend of the reactions and the formation of entirely different end-products than would otherwise have resulted. And velocities may be varied greatly by such factors as temperature and catalytic agents.

If in the comparatively simple cases of associated simultaneous reactions with which we are acquainted in non-living matter, relative velocities may so modify the results, we can readily realize of what tremendous importance regulation of this matter must become in living protoplasm where doubtless vast numbers of chemical reactions and interactions are going on at the same time. In fact, could we locate such a time-regulating factor in the germ-cell it would seem that we had accomplished a long stride toward an understanding of the controlling and coordinating mechanism which insures the appearance of just the proper substance at the right time in morphogenesis. It would constitute a qualitative as well as a quantitative regulator, for by determining quantity at any given time it determines what the next chemical reaction will be, and hence in the very doing of this, it necessarily conditions the chemical outcome of that reaction. As we have seen, temperature and catalytic agents are important factors in modifying the velocities of reactions in ordinary chemical processes, and inasmuch as under normal conditions of development the temperature factor is a fairly constant one, we are left to face the question as to whether in protoplasmic phenomena there is anything to correspond to catalyzers. Such substances we find in the enzymes.

In any epigenetic conception of the germ-cell—and this in greater or less degree seems to be the only plausible one to-day—we are forced, in explaining morphogenesis, to postulate the existence of some time-, quantity- and quality-controlling mechanism. The one evident class of substances in the germ-cells which can fulfil the necessities of the case are the ferments. For since they will determine the velocities of chemical reactions they must in consequence control the quantitative relations of the cell chemistry at any given unit of time. But from the very fact that where a large number of associated reactions are going on simultaneously, these quantitative relations at given stages of the chemical interchanges must profoundly influence qualitative results, we can not but conclude that this initial control of velocities must condition the qualitative results.

If we regard the chromosomes as centers of such a series of velocity-controllers, or, in other words, as sources of various enzymes, we can at once appreciate the necessity for having them so accurately balanced off in size and particularly in their quantitative relations one to another. For since the velocity of the reaction in a fermentable substance is determined not only by the presence of the ferment, but also by the amount of it, the quantitative relations of the ferments to one another would have to be very accurately maintained.

There is no obstacle in the way of supposing, furthermore, that if we regard ferments as of nuclear origin, the cytoplasm of a given tissue may not modify the ferment, as it itself takes on the necessary modifications for its own specific functions. We have good evidence that the production of ferments can be modified by even the substratum on which living organisms grow, and such a relation as this, close as it is, is certainly less intimate than that existing between nucleus and cytoplasm. For example, certain molds cultivated upon starch form diastase, but if provided with albumin they will produce instead a proteolytic ferment. Moreover, by gradually altering their other nutriments, yeasts can be made to utilize after a time various foreign compounds.

If, as all evidence indicates, ferments operate as catalyzers, then we must not forget that it is the very general belief among chemists that catalytic agents do not initiate the chemical reactions with which we find them associated, but that they only tremendously accelerate such reactions, or in a few known instances retard them. Since the nature of the building material must determine fundamentally the nature of the thing built, we must look outside the enzymes for much that will determine the peculiar individual outcome of the developmental processes. Leaving out of consideration for the present other functions the chromosomes may subserve, we might regard them as a sort of gauge for the feeding out of enzymes at the proper rate to bring about proper velocity reactions in the other cellular constituents, and perhaps regard the whole matter of mitosis and exactness in chromosomal distribution as a mechanism by which a quantitative metabolic regulation is maintained.

Looked at this way, the physical basis of heredity could not be considered a series of equipotent units, but rather it must be regarded as being composed of systems of units of different orders of organization and different degrees of coordination. Alterations in the configuration, constitution or relative positions of the unit constituents which represent the links of the main protein chain or ring, for instance, would precipitate much deeper-seated changes than would replacement of side-chains by those of different type, and such replacements would, in turn, doubtless appear objectively as differences of greater degree than those resulting from shifts in the composition or configuration of the individual side-chains.

If we consider that the supplying of the proper amounts and kinds of ferments is one of the important functions of the chromosomes, then we may suppose that in biparental inheritance each set of chromosomes is operating, probably catalytically, on a series of fundamental cell constituents that are largely common to both lines of ancestry; and that slight constitutional or configurational differences in corresponding enzymes bring about individual differences such as we recognize in the adult. We have already seen that different ferments

within certain limits may act on the same substance and yield different results; consequently, in the intrusion into the egg of slightly altered enzymes in the chromosomes of the male, we should expect corresponding structural modifications to result.

Any influence which could effect constitutional or configurational changes in other essential constituents of the germ-cell would doubtless produce corresponding alterations in the adult. It is probable that not only changes of nuclear origin are reflected on to the cytoplasm, but that, conversely, cytoplasmic alterations may affect the nuclear constituents, for we have already seen how even the substratum may modify the enzyme factors in entire organisms such as molds and yeasts. Furthermore, there is no reason apparent why if the differences, no matter how produced, are modifications in the fundamental constitution or stereometry of the material affected, they should not persist permanently in the new germ-cells.

It would seem, in fact, that in the permanent effects of such reciprocal influences as here depicted for nucleus and cytoplasm, we might be able to account in large measure for the accumulations which have step by step been grafted on to the primitive protoplasm in its epigenesis toward the complex conditions of to-day, or in other words, in its racial evolution. Moreover, it is conceivable upon this basis how in later stages of phylogeny, as new chemical configurations or new chemical substances were developed, some of these could bridge back into relations with more primitively established substances and thus bring about ontogenetic short-cuts in development, or how, on the other hand, these abridgments might result in part from alterations in the more primitive molecular configurations.

I may seem to have run on to great length aimlessly, but I would simply point out that if a mere shift of tags is made, what in this earlier paper I regarded as chromosomal enzymic forci become "genes," and the account sounds much like a modern attempt to explain the action of genes through enzymic behavior.

I have not carried you through all this repetition, however, merely for the satisfaction of dressing up old stuff in new guise, but because I think that in its line of argument one may find some germ of thought about what I started to discuss, the germinal background of somatic acquirements. Among other things the citation of the relations of yeasts and molds to their substrates and the possibility of one eventually radically modifying the other may not be without significance in the chromosome-cytoplasm relation.

In what respects the germinal predispositions which permit of the appearance of so-called acquired characters under special conditions differ from the predispositions that lead to the appearance of inherited characters under usual conditions, no one knows. About all we can say is that in case of what is commonly regarded as the inherited character, the character is usually capable of reappearing in successive generations without being called forth each time by a specific environmental factor. But since, as we have seen, the potentiality of acquired as well as of inherited characters must in some manner exist in germinal protoplasm, the difference would seem to be one of degree or of fixation rather than of kind.

We have no reason to believe that the living substance of the germ-cells has any mysterious powers that are not shared by any or all of the somatic cells. In tissue cells we know that changed internal relations or unusual environmental stimuli may result in such diverse modifications as those of growth, hypertrophy, atrophy, hyperplasia, metaplasia and what not. Just as excessive exercise leads to overdevelopment of a muscle, so increased strain in bone leads to increased growth of bony tissue; if one of a pair of organs (lung, kidney, thyroid) is lacking or is destroyed the other soon adjusts and performs the function of the pair combined.

In many compensatory adjustments, as of enlargement of the hemolymph glands and bone-marrow following removal of the spleen, the compensating organ is not in direct connection with the one which is disturbed or missing. It seems probable that the inciting agent is carried by the circulating fluids of the body, although in higher animals one may have to reckon with the nervous system. If, however, a serum-borne agent may incite compensatory hypertrophy or other changes in tissue-cells, may not some serum-borne agents stimulate germinal protoplasm to additive functioning or other changes? It seems improbable that the germ-cells, bathed in the same fluids, nourished by the same food, stand wholly apart. If we had but a single side-chain in common between a protein of a somatic tissue and a protein of the germ, then anything that could affect one might well be expected to affect the other. In the endocrinal secretions alone one sees a series of powerful substances circulating through the body and producing profound effects in any or all of its parts. Both clinical and experimental evidence reveals that increase or diminution of an endocrine gland may be followed by marked alteration of bodily structure or function. When one sees how sex endocrines may experimentally be made to override genetic constitution itself in determining sex, he is tempted to regard the very male- and female-determining genes as possibly intracellular endocrinal structures. Since change in an endocrine gland may produce permanent changes in various tissues, may there not be germinal homologues of such tissue which may likewise be modified, particularly if repeated generation after generation? In an earlier paper[14] I have expanded upon this theme, especially as it might have significance in orthogenesis. Any

[14] *Am. Nat.*, 56, March–April, 1922.

internal or external agent which could affect particular constituents, nuclear or otherwise, of the somatic cells should, on the face of things, be able to influence homologous elements in germ-cells.

My own experiments in this connection, with immune sera, have already been laid before this society and I shall not subject you to the tedium of recounting them again. All I wish to say at present is that the experiments both on the effects of lens antibodies and on transmission of induced immunity are being repeated, this time with inbred stocks, and in the case of the lens work, with individual lens proteins. A small though sufficient number of successes are being secured from time to time to keep me encouraged and of the same opinion as that expressed in my earlier work. I still believe that the serological reactions of the body afford one means of breaking in upon the germ.

What apparently would have to happen to have an acquired become an inherited character would not be the germinal creation of the capacities for its appearance—these must already exist or it could not appear—but some sort of germinal fixation that establishes it as part of the more habitual expression of the germ-plasm. We commonly conceal our ignorance of the matter, to be sure, by talking about the "plasticity" of the organism, but this very plasticity must have genic implications, for the fact remains that the organism has the inherent capacity for acquiring the somatic modification.

That profound shifts in the organization of the germinal protoplasm may occur, particularly in the order of the appearance of characters in individual development compared with the order of their evolutionary acquisition, is evident when one regards the frequent precocious appearance of an adaptive mechanism far in advance of the conditions under which it is to operate. The eye of the unborn mammal, for example, develops long before it encounters the external agent light upon which it depends for its very significance. Yet, if our conceptions of evolution mean anything, the vertebrate eye must originally have developed in some functional cooperation with light, no matter whether we regard light as a causative or merely as a selective agent. Likewise the mammalian placenta, though among the latest of acquisitions in phylogeny, is one of the first things established in ontogeny. The point I would make is that, as time goes on, adjustments do come to pass in germ-plasm which may alter the chronological relations of hereditary acquisitions, and that this indicates that germ-plasm is not a fixed, inadaptable thing.

It would seem not improbable that any acquired adjustment, based on the casual potentiality of the genes of the organism, repeated generation after generation, would foster successive adapted generations of individuals until such casual reactions of the genic complex became its customary reaction. Obviously both types of potentiality must reside in germinal protoplasm. At present I see no explanation of how casual potentialities of the germ-plasm which permit of the somatic acquirement of characters become ingrained in the mechanism which underlies the more independently recurrent characters called hereditary, unless it is to be found in the quantitative changes in genes or of genic potencies. Evidence of such changes seems to be slowly accumulating. Once concede that the constitution of the gene can wax or wane and the way is open to the conception of how this might be induced through nutritive, toxic or functional means. It may be that when we learn more about the protoplasmic basis of ordinary hypertrophy, atrophy and habit-formation we may also see our way toward an understanding of the origin of inherited adaptations. Until we do so possibly we shall remain in ignorance of this most elusive attribute of all living things.

THE NATIONAL ARBORETUM[1]

By Dr. FREDERICK V. COVILLE
ACTING DIRECTOR

THE National Arboretum is an institution for the increase and diffusion of knowledge concerning trees. The National Arboretum Act, approved by President Coolidge on March 4, 1927, states that the purpose of the arboretum is research and education concerning tree and plant life and that in order to stimulate research and discovery the National Arboretum shall be under scientific direction.

To study the living tree, to breed new kinds, to select the best among them, to develop methods of propagating them, to show to what soils and to what situations and to what special purposes they are best adapted, such is the field of experiment and discovery to be occupied by the National Arboretum. The human race has bred sheep and cattle and horses, but not elephants. The elephant is too big, too wild, too long-lived. The human race has bred wheat and potatoes and apples, but not timber trees. They also have seemed too big, too wild and too long-lived. But the time has come to begin.

A strange case of the superiority of an individual

[1] Address delivered on January 17, in Washington, D. C., before a meeting of the American Society of Landscape Architects.

tree has recently come to light, in a most unexpected way. One of the soil chemists of the Department of Agriculture, Mr. W. O. Robinson, amuses himself at home by making bows and arrows and shooting them. He has made bows of every kind of resilient wood the world affords, so far as he has been able to obtain it. One of the very best woods for bows is yew, which our own ancestors in western Europe used when they were savages, and which the Klamaths, Modocs and other Indian tribes of our northwest coast used until we brought them the rifle. But a yew bow is not durable. It loses much of its elasticity and strength after a few years of alternating heat and cold. There is another American wood, however, from which are made bows of such superiority and such lasting qualities that in the old Indian days they were handed down from father to son. This wood is the Osage-orange, the *bois d'arc* of the early French trappers, corrupted into *bodark* by Mississippi Valley frontiersmen, and made into a scientific name *Toxylon,* or bow wood, by one of the early botanists. The best commercial supply of Osage-orange comes from Texas. With this wood Mr. Robinson has made many bows of remarkable performance, a typical bow of forty-nine pounds pull weighing 25.1 ounces. Last year Mr. Robinson made the discovery that from an Osage-orange tree in an old hedge on the farm of a friend near Rockville, Maryland, he could get equal strength and elasticity with little more than half the usual weight of wood. From this tree he made a bow weighing 17.7 ounces, which has a pull of sixty-two pounds. With this bow he shot an arrow 341 yards, a far greater distance than any authentic American record up to that time. No one knows how far, in the hands of a more expert bowman, this Osage bow will shoot.

The stump of this tree is still alive. At the arboretum we shall try to propagate and perpetuate it, by cuttings or by grafting, and if we can find another Osage tree of equal superiority we will cross-breed the two and develop a whole population of these tree aristocrats. One can not foretell the possible industrial uses of such a wood. It is suggested, however, by way of warning, that if golf sticks are ever made from the Rockville Osage, it may become necessary for golf clubs to double the length of their holes.

The search for superior trees of every kind and the making of experiments with them will occupy, undoubtedly, a large part of the arboretum's activities in future years. The beautification of American homes and the beautification of parks for purposes of outdoor relaxation and recreation is dependent largely on trees. Every one knows the beauty of the Japanese flowering cherries in Potomac Park. Even in Japan, last spring, I saw no more impressive display of cherry blossoms than is afforded by the mile stretch of Yoshino cherries around the borders of our Tidal Basin, that most gracious gift from the City of Tokyo.

These Japanese cherries have set us thinking about the little-appreciated beauty of some of our own wild trees. Two or three years ago I suggested to Colonel Grant the desirability of preparing for our public parks an extensive display of our American sweet-scented wild crab. This is the tree that the good Lord evidently intended for our national flower, but we have been slow to appreciate the divine gift and to provide for it the place of honor it deserves. Our wild crabs are of several species. They extend from New York to Minnesota and southward to Florida and Louisiana, and they are beloved by all who know them. At the National Arboretum we not only shall give them a place of honor, but we shall select the most outstanding individuals among them, and these we will cross-breed, making selections again among the progeny until we have developed a wide range of varieties, more beautiful even than the wild trees and more fully adapted to the purposes of landscape art. I can not believe, however, that we shall ever develop a cultivated crab more fragrant than the wild ones.

For some reason which is unexplained, Providence did not allow the wild crab to enter New England, the home of our early poets. In consequence its beauty and its fragrance long remained unsung, but ask the man from Des Moines or from Tallahassee if he knows the wild crab, and you will get a new lesson in the appreciation of nature.

Although many of the botanical experiments and discoveries made at the arboretum will be of value chiefly to horticulture and to landscape art, the most important work of the arboretum will find its application in forestry. In a recent address in New York, before the Garden Club of America, I made a statement which I may be permitted to repeat here:

> It is well known to American foresters that our original stands of timber are fast disappearing. White pine is already gone. At the present rate of cutting, the original stand of southern longleaf pine is expected to last eight years. The irregular process of natural reseeding will replace, ultimately, some of the timber that we have cut. We shall be compelled, however, before long to grow trees as a crop, just as we grow corn or wheat or potatoes. When we begin to plant trees as a timber crop, should we plant wild seedlings or should we plant improved strains, just as we now plant improved strains of corn, wheat and potatoes? There can be but one answer. When we go to the expense of growing trees as a crop we shall plant improved kinds just as soon as we have the improved kinds to plant. It is the primary function of the National Arboretum to

develop improved kinds of timber trees, by selection, by hybridization and by comparative tests of wild seed, selected seed and hybrid seed. We have every confidence in the outcome of such research and experimentation, for the tree is only a plant and that kind of experiment has succeeded with every other plant to which it has been scientifically applied. A white pine so improved in its rate of growth that it will reach in fifty years or in sixty years the size that now requires eighty years would repay the cost of the National Arboretum for a generation.

The members of the American Society of Landscape Architects here present may with propriety be asking for what purpose is this address delivered before this society, for although much has been said about the relation of the arboretum to forestry and to horticulture, little has been said about its relation to landscape art. I must confess that at the present time I am chiefly concerned not with what the National Arboretum can do for landscape art but with what landscape art can do for the National Arboretum.

The arboretum is still in the land-purchase stage. Its actual operation has not yet begun. The secretary of agriculture has purchased, or is in process of purchasing, 268 acres. He expects to make additional purchases which, with other land already owned or claimed by the government, will make a total of approximately the thousand acres originally planned for the arboretum. The site contains several elements of great dignity and beauty: Mount Hamilton, covered with a growth of mature hardwood timber and overlooking the city, with an excellent view of the Capitol, the Washington Monument and the Arlington hills in Virginia; Hickey Hill, with fine views up the Anacostia River and down it to the Potomac; the curve of the upper Anacostia, bordered with wild-rice and sweeping around to the old deer lick at Licking Banks, and the waterlily gardens at Kenilworth, one of the most beautiful sights in the city of Washington.

The National Arboretum is to be an outdoor laboratory for scientific research on trees. The materials for research are the trees and shrubs that will be planted in the area. It is planned to bring there every kind of tree that will thrive out of doors under this climate. There will be buildings, greenhouses and nurseries. Broad highways will border the arboretum, and it is expected that one of the principal entrances to Washington will pass through it. The parts of the arboretum in contact with these principal streets, or in view from them, should be developed with all the natural beauty that the situation affords. The greenhouses, the nurseries, the experimental plantings in straight lines and the every-day work with spades and hoes should be secluded from the main highways.

In the development of the National Arboretum the secretary of agriculture already has the hearty cooperation of the Garden Club of America, the American Association of Nurserymen and many scientific organizations. He would greatly appreciate the cooperation and advice of the American Society of Landscape Architects.

OBITUARY

JULIUS BUEL WEEMS

DR. JULIUS B. WEEMS, chief chemist of the Agricultural Department of the State of Virginia, died suddenly on Saturday morning, January 25, at his home in Ashland, Virginia.

Dr. Weems had been with the state for fifteen years and was widely known as a chemist. His contributions to scientific journals, in the form of articles on agricultural and chemical problems, added to the reputation which he had as a consulting and analytical chemist, and a student of farm questions before he came to the Department of Agriculture.

He was born in Baltimore, August 27, 1865, the son of Edwin Dawson Weems and Rosetta Norman Weems. Following his graduation from Maryland Agricultural College in 1888, he studied at Johns Hopkins University for two years. In 1894, while a fellow in chemistry at Clark University, he received his degree of Doctor of Philosophy. For the next ten years he was professor of agricultural chemistry and chemist of the experimental station at Iowa College.

Dr. Weems was a member of the Society of American Bacteriologists, the American Chemical Society, and a fellow of the American Association for the Advancement of Science.

W. C. J.

AUGUST TOBLER

THE death is announced on November 23 of Dr. August Tobler, the director of the Geological Section of the Natural History Museum, Basel, Switzerland. Dr. Tobler was a geologist of international reputation. He did much work in the Netherlands East Indies, his principal publication on that region being "Djambi-Verslag," which consists of a volume of 585 pages of text and 19 plates and an atlas of 9 geological maps, structure sections and a table of the different geological formations. Dr. Tobler also made studies of the geology and paleontology of northern South America, especially Venezuela and Peru. The high quality of Dr. Tobler's work is recognized by every one familiar with it. Besides the respect due him because of his scientific attainments, he endeared

himself to his scientific colleagues by the help that he was ever ready to render them in their investigations. His death causes a severe scientific loss and removes a personality which had extended its kindly influence to all the continents.

T. W. V.

KAMAKICHI KISHINOUYE

DR. KAMAKICHI KISHINOUYE, professor emeritus of fisheries in the faculty of agriculture in Tokyo Imperial University, died on November 22, 1929, at Chengtu, Province of Szechuan, China, while collecting fresh-water fishes in the upper part of the Yangtze-Kiang. He started from Tokyo last August on an expedition to collect fishes along the Yangtze-Kiang and had been successful in obtaining much material, but was attacked with some alimentary disorder and died on November 22.

Professor Kishinouye was in several lines of work one of the leading scientific men of Japan. He published a number of papers on the Alcyonarian corals, paying special attention to those that produce coral of commercial value. He studied numerous problems of the life history of Japanese fishes and was especially well known for his investigations of the scombroid (mackerel) group. He was the author of a volume entitled "Contributions to the Comparative Study of the So-called Scombroid Fishes," published in the *Journal* of the College of Agriculture of the Imperial University of Tokyo, Volume 8, No. 3, 1923.

Professor Kishinouye attained high scientific recognition in his native country, being a member of the Imperial Academy of Sciences. Besides being a man of scientific distinction he possessed great personal charm and was a good example of the courteous Japanese gentleman of the old school.

T. W. V.

RECENT DEATHS

DR. LEA MCILVAINE LUQUER, tutor and associate professor of mineralogy at Columbia University from 1887 to 1925, died on January 30, at the age of sixty-four years.

DR. J. FRED MOHLER, for thirty-three years professor of physics at Dickinson College, died on January 28 at the age of sixty-five years.

WILLIAM WALLER CARSON, professor emeritus of civil engineering at the University of Tennessee, died on February 7, at the age of eighty-four years.

DR. THORBURN BRAILSFORD ROBERTSON, professor of physiology and biochemistry at the University of Adelaide, died on January 27 at the age of forty-five years.

SIR GEORGE DANCER THANE, the British anatomist, died on January 14 at the age of seventy-nine years.

DR. YOSHIAKI OZAWA, professor of paleontology at the Imperial University of Tokyo, died on December 29. Dr. Ozawa had returned to his work in Japan this last summer after two years of foreign study in Europe and the United States.

DR. BENEDIKT DYBOWSKI, professor of zoology at Lwow University, died on February 1, at the age of ninety-five years. After being exiled to Siberia for participating in the 1863 insurrection in Poland he undertook a study of fauna from Lake Baikal to Kamchatka.

SCIENTIFIC EVENTS

AGRICULTURE IN THE BRITISH EMPIRE

THE nineteenth annual report of the British Development Commissioners for the year ended March 31, 1929, deals, according to a report in the London *Times*, with a large range of questions affecting agriculture, rural economy, fisheries and harbors, the compulsory acquisition of land for road improvement, and the financial position of the development fund.

The total advances from the development fund recommended during the year amounted to £394,752, compared with £383,652 in 1927–28. The administrative expenses of the commission amounted to £9,315, compared with £9,887 in the preceding year.

Since the commissioners' last report eight bureaus have been established in accordance with the recommendation of the Imperial Agricultural Research Conference in 1927. Their purpose is to collect and disseminate information of a scientific character on various branches of agricultural research. Their establishment so soon after the meeting of the conference is a matter for congratulation, the commissioners remark, because of the assistance they will give to scientific workers in different countries. But the benefit to individual and isolated workers is not the only reason for which the bureaus are welcomed. Since they are the result of joint action taken by a number of empire governments, will be controlled by a representative empire committee, and financed by empire contributions, the bureaus are to be welcomed as a first—and it is to be hoped only a first—step in a policy of cooperation by the governments of the empire for the promotion of agriculture by enlisting scientific aid.

It is estimated that £22,000 per annum will be required to maintain the bureaus, and this sum is being raised by proportionate contributions. The contribu-

tion of England and Wales has been assessed at £3,-125, and that of Scotland at £500. The commissioners have recommended payment of these home contributions from the development fund, in view of the advantages which British agricultural investigators will derive from participation in the scheme.

While it is unquestioned, the commissioners add, that advantages will accrue to agricultural science in this country from the creation of the bureaus, it is anticipated that still greater advantages will be derived from them by oversea workers. It is not only because of the dominating position which agriculture holds in the national economy of oversea countries that their representatives pressed for the bureaus. If this had been the main reason the proposal would have been made many years ago; for in some countries at least the need of a central organization has long been recognized. The immediate cause of the demand made at the conference was the clear realization by empire workers that the mother country is now in a position to offer them substantial aid. The institutions which have been built up as a result of the creation of the development fund in 1909 are taking a leading place in the application of scientific research to agriculture and Britain can offer, as twenty years ago it could not, much valuable guidance for those attempting to improve agricultural practice in the oversea empire.

CARNEGIE-AUSTRALIAN-HARVARD EXPEDITION TO NORTHWESTERN AUSTRALIA

UNDER the combined auspices of the Carnegie Institution of Washington, which made the original grant, the Australian National Research Council, which made a generous supplementary grant, and the Museum of Comparative Zoology, Harvard University, Professor Hubert Lyman Clark, of the museum, has just completed an important investigation of the echinoderm fauna of the northern and western coasts of Australia. Leaving Cambridge in the spring, accompanied by Mrs. Clark, who has served as artist and general assistant, Dr. Clark attended the Pan-Pacific Scientific Congress in Java in May, as delegate from Harvard University and the American Society of Naturalists. After three weeks in Java, he and Mrs. Clark went to Darwin, N. T., where they arrived on June 13 and a few days later were joined by Mr. Arthur A. Livingstone, of the Australian Museum, who continued with them until the party reached Perth, a most willing, competent and congenial assistant.

At Darwin the collecting was poor, especially along shore, owing to the large amount of sediment in the water which tends to form a muddy deposit wherever it is not kept scoured off by tidal currents. During the more than six weeks spent at Darwin only sixty-five species of echinoderms were found, though excursions to points from six to thirty-five miles distant were undertaken. Of the sixty-five species, not a dozen could be called at all common. On July 29 the party left Darwin and went to Broome, W. A., with brief stops at Wyndham and Derby on the way. These stops were long enough to show that local conditions at those ports were even more unfavorable for echinoderms than at Darwin, owing to the muddy water. At Broome, however, where two months were spent, the water is ordinarily very clear and marine life is extremely abundant. As local conditions are very varied and the tidal range very great (up to thirty feet and even more), Broome is a paradise for the marine zoologist. As the pearl-shell fishery is the one essential occupation, the local boats with their divers provide an unusual and important means of securing zoological material. Excursions were made from Broome, to Cape Levéque lighthouse, 142 miles to the northeast, and to La Grange Bay, Anna Plains and Wallal, 150 miles to the southwest. Altogether more than 170 species of echinoderms were secured in this region, many of which are apparently as yet undescribed.

On October 1 the party left Broome for Perth. There was a short stop at Onslow which yielded nothing and a day at Geraldton which yielded a few interesting species. At Perth, a stay of nearly three weeks, with important excursions to Rottnest Island and Bunbury proved most rewarding. The cooperation of the museum, university and government officials was generous and helpful to the last degree. Similar but briefer stops were made at Adelaide, Melbourne, Hobart, Sydney and Brisbane, and at all these places the courtesies extended knew no limits and the cordial cooperation shown was overwhelming. At Adelaide and Melbourne opportunities were given for examining both in the field and in the museums many of the fossil echinoderms of Australia, chiefly Tertiary. The cooperation of the Australian Museum at Sydney has been constant and most cordial during the whole investigation. Nothing has been left undone by Australian scientists which could further Dr. Clark's researches in any way. As a result of this hearty cooperation, it has been possible to study in life, under normal environmental conditions, some three hundred species of Australian echinoderms, about three fourths of which were seen on the northwestern coasts of the continent, a region hitherto a *terra incognita* to the marine zoologist.

THE GEORGE WILLIS PACK FORESTRY FOUNDATION OF THE UNIVERSITY OF MICHIGAN

THE promotion of practical forest land management in the broadest sense of the term is the object

of a gift of $200,000 made by Charles Lathrop Pack, of Lakewood, New Jersey, to the University of Michigan. This fund is to be known as the George Willis Pack Forestry Foundation in memory of the donor's father, and the income from it will be used by the school of forestry and conservation in carrying out the purpose of the foundation.

In establishing the foundation, Mr. Pack stated that the school of forestry and conservation was chosen as the medium through which to carry out his plans for the advancement of forestry in this field "on account of its prestige, its facilities for teaching the broad principles of all phases of practical forestry and its experienced staff."

The income from the foundation is to be used for the salary and expenses of an experienced forester to be known as the George Willis Pack professor of forest land management, and for such other expenditures as may be necessary in carrying out the purposes of the foundation. In accordance with the wishes of the donor, who is keenly interested in the practical application of forestry, it is expected that the holder of this professorship will devote the larger part of his time to furthering the practice of forestry in the woods rather than in the classroom. He may, however, assist in the instructional activities of the school, and will doubtless work particularly with graduate students. He will be expected to spend considerable time in travel in order to keep in close touch with forest conditions and practices in other parts of the country and to make the results of his experience as widely available as possible.

In commenting upon the gift, Dean Samuel T. Dana, of the school of forestry and conservation, stated that the income from the George Willis Pack Forestry Foundation will be used, in accordance with Mr. Pack's wishes, for two major purposes—to develop the wild land properties owned by the university, and to cooperate with forest land owners in bringing about the practice of forestry and more profitable methods of forest management of privately owned lands. "The University of Michigan," said Dean Dana, "has three small areas of forest land in the vicinity of Ann Arbor, which are used both as outdoor laboratories for the instruction of students and for investigative purposes, and which are already under careful management. In addition to these, the university owns approximately 6,300 acres of wild lands in the northern part of the state."

In making his gift, Mr. Pack pointed out that the Biological Station at Douglas Lake in Cheboygan County contains some 3,300 acres on which forestry measures can advantageously be undertaken to supplement the work already under way by the zoologists and botanists at the station. Situated within a few miles of the city of Cheboygan and in the immediate vicinity of several summer camps and serving as headquarters for the biological station with a summer attendance of approximately one hundred students from all parts of the country, it is particularly well located for demonstration purposes.

Excellent opportunities for forest management are also offered by the Chase S. Osborn Preserve on Sugar Island in the St. Mary's River, presented to the university last fall by Ex-Governor Osborn. This tract of some 3,000 acres was burned perhaps fifty years ago and is now covered with a well-stocked second growth consisting of a wide variety of both hard and soft woods.

GIFTS TO THE SCHOOL OF FORESTRY OF YALE UNIVERSITY

YALE UNIVERSITY has announced three new gifts to the School of Forestry to be devoted to education, experiment and demonstration in applied forestry. The Charles Lathrop Pack Foundation given last year by Mr. Charles Lathrop Pack, of Lakewood, New Jersey, has been increased to $325,000. A gift of $100,000 to endowment has been received from Mr. and Mrs. Starling W. Childs, of New York. Mr. George Hewitt Myers, of Washington, is transferring to the school a tract of forest land in Tolland and Windham Counties, Connecticut, aggregating in area nearly 8,000 acres. These gifts, all closely related in purpose, constitute a new project, supplementing and greatly strengthening the work and facilities of the school of forestry in instruction and in advancing the knowledge and practice of forestry. Announcement is also made of the appointment of Mr. Nathan D. Canterbury, formerly State Forester of Louisiana, as director of the new Yale Forest, on the Charles Lathrop Pack Foundation.

According to Dean Henry S. Graves:

> The new gifts will enable the school of forestry to extend its field activities, both in connection with the training of students and in providing demonstrations of practical forest management. The new forest is large enough for a practical operating unit and for the demonstration of all the various activities involved in the management of a wooded estate of this size and character. Protection from fire, maintenance of roads, trails and other improvements, cutting of timber under skilful methods, forest planting, marketing of forest products, administration of wild life and many other features of applied forest management will be demonstrated at the Yale Forest. The undertaking will give special emphasis to the training of students and experimentalism in applied forestry; it provides also for owners of forest estates to see the practical results of forestry.

The forest is located in the towns of Union, Ashford, Eastford and Woodstock, in northeastern Connecticut. It

has been protected for many years and is well stocked with hard woods, pine and hemlock. There is enough merchantable timber to permit economic forestry operations at once. The educational phases of the work on the forest will be progressively developed, as the details of administration of the property are perfected.

The director of the forest, Mr. N. D. Canterbury, graduated from the Yale School of Forestry in 1922. He has been associated with various enterprises, both in business and in public service.

THE CHILEAN NITRATE OF SODA NITROGEN RESEARCH AWARD

ABOUT two years ago the Chilean Nitrate of Soda Educational Bureau of New York made available an annual appropriation of $5,000 to be distributed in the form of one or more prizes for outstanding research in this country and Canada on nitrogen in its relation to soil processes and plant growth. The above sum has been placed in the hands of the American Society of Agronomy, the award being administered by a special committee of the society.

The first awards were made in 1928. The committee held then that it was advisable to select older investigators whose previous work in advancing knowledge of nitrogen in relation to plant growth has been beyond question. Professor J. G. Lipman, director of the New Jersey Agricultural Experiment Station, Professor T. L. Lyon, chief of the soil department, Cornell University, Professor E. B. Fred, division of agricultural bacteriology, University of Wisconsin, and Professor F. T. Shutt, dominion chemist, Ottawa, Canada, were those selected. In connection with the announcement of the selections for 1928 the committee suggested that subsequently it would be appropriate to consider and perhaps even give preference to those younger investigators who have made recent contributions to the subject.

The second award, announced at the 1929 meeting of the society, was divided among three investigators, namely, Professor C. A. Mooers, director of the Tennessee Agricultural Experiment Station; Professor P. L. Gainey, soil bacteriologist of the Kansas Agricultural Experiment Station, and Professor S. A. Waksman, microbiologist of the New Jersey Agricultural Experiment Station.

In order to make clear the nature of the investigations that will be considered for the award in 1930 the committee feels that it is desirable to publish a brief statement at this time.

Investigations will be considered which deal with the physical, chemical or microbiological transformation of both organic and inorganic nitrogen in the soil, the absorption and utilization of nitrogen by plants and nitrogen fertilizer in any of its aspects. The committee will give particular attention to those contributions which have been published within the past year or two. Investigations of longer standing will also be considered provided the authors are still actively engaged in the work.

Investigations dealing with any phase of nitrogen in relation to soils and crops may be called to the attention of the committee either by an author himself or by any one else. The award, which may be made either to one or more investigators, will be recommended to the society, according to the judgment of the committee.

The committee takes this opportunity to solicit nominations of candidates for the 1930 award. The nominations should be made in writing and may be addressed to any member of the committee. Although it may be difficult to select the "most" outstanding contribution to such a complex subject as nitrogen in its relation to soils and plants, the committee hopes that it will at least be able to select one or more investigators who have made a notable contribution to the scientific understanding of the rôle of nitrogen in soils and plants. It is expected that the award will be used in such a way as to lead to still higher scientific activity.

The award is open to investigators irrespective of their connection with the American Society of Agronomy. The investigation may have been made at a university, state experiment station, government institution or privately.

R. W. THATCHER,
C. B. WILLIAMS,
W. P. KELLEY,
R. I. THROCKMORTON,
R. BRADFIELD,
S. A. WAKSMAN, *Chairman*
Chilean Nitrate of Soda Nitrogen Research Award Committee

SCIENTIFIC NOTES AND NEWS

A BRONZE memorial plaque, picturing Mr. and Mrs. Thomas A. Edison, was unveiled at Fort Myers, Florida, on February 11, in celebration of Mr. Edison's eighty-third birthday. The plaque was erected in the city park by a committee of civic workers and will be illuminated perpetually.

DR. W. M. DAVIS, emeritus professor of geology at Harvard University, celebrated his eightieth birthday

on February 12 at Tucson, Arizona, where and in California he has been actively engaged in geological work.

AT a meeting of British botanists in London, held to receive the report of the executive committee on the progress which has been made in organizing the International Botanical Congress, which will be held in Cambridge from August 6 to 19 under the presidency of Professor A. C. Seward, a message of congratulation was sent to Dr. Sydney Howard Vines, F.R.S., Sherardian professor of botany in the University of Oxford, and fellow of Magdalen College from 1888 to 1919, who celebrated his eightieth birthday on December 31.

DR. IRVING LANGMUIR, associate director of the research laboratory of the General Electric Company, has been awarded the Willard Gibbs Gold Medal of the Chicago Section of the American Chemical Society for 1930, for "fundamental work on atomic hydrogen and on surface relations and also on electrical discharge phenomena. Also for his contributions of great importance to nearly all branches of physical chemistry, including high vacuum technique, electronics, thermochemistry and catalysis. And lastly for his presentation of a theory of atomic structure."

THE presentation of the American Iron and Steel Institute Medal awarded in October to Dr. Elmer A. Sperry, president of the Sperry Gyroscope Company, was made at a meeting of the board of directors at the Metropolitan Club on January 31. The medal, which was founded by the institute in memory of its founder and first president, Elbert H. Gary, was awarded for the first time to Dr. Sperry especially for his work on the non-destructive detection of flaws in steel rails. Mr. Charles M. Schwab, chairman of the board of the Bethlehem Steel Corporation and president of the institute, made the presentation.

AT the annual general meeting of the Royal Meteorological Society on January 15, Sir Richard Gregory, the president, presented Dr. G. C. Simpson, director of the Meteorological Office, with the Symons's Gold Medal, in recognition of the value to meteorology of Dr. Simpson's researches on thunder-storms and terrestrial radiation.

A BRONZE *bas relief* of Dr. C. H. Kauffman, professor of botany and director of the herbarium, was presented to the University of Michigan on January 24. It is the gift of twenty-eight doctorate students of Dr. Kauffman, which group includes those who have taken the degree of doctor of philosophy and those who are at present working under him. The plaque was made by Mr. Carleton W. Angell, of the University of Michigan, and it was sent to Munich, Germany, to be cast in bronze. Dr. G. Carl Huber, dean of the Graduate School, presented the portrait to the university, and it was accepted by President Alexander G. Ruthven. It will be placed in the herbarium library in the museums building.

DR. SAMUEL THEOBALD, clinical professor emeritus of ophthalmology in the Johns Hopkins University School of Medicine, was honored by the ophthalmologic section of the Baltimore City Medical Society on January 23, the fiftieth anniversary of his discovery of the use of boric acid in direct application to the eye.

DR. H. S. FAWCETT, professor of plant pathology in the Citrus Experiment Station of the University of California, was elected president of the American Phytopathological Society at the recent Des Moines meeting.

DERWENT WHITTLESEY, assistant professor of geography at Harvard University, has been appointed editor of the *Annals* of the Association of American Geographers.

PROFESSOR ARTHUR C. HARDY, of the department of physics of the Massachusetts Institute of Technology, has been chosen by the Society of Motion Picture Engineers to be chairman of its committee on standards. This committee will consider the proposed increase in the width of motion-picture film. The standard film is thirty-five millimeters wide. With the advent of the talking motion picture it was necessary to use about one tenth of an inch from one side of the film for the "sound track," thus narrowing the space available for the picture area. Recently films of widths ranging from fifty to seventy millimeters have been proposed and some experimental pictures have been shown. The object of wider films is to permit the projection of pictures of full stage dimensions.

DR. A. A. LEVINE, who for the past six and a half years has been on the staff of the department of chemistry of the State College of Washington, has resigned to accept a research position with the Roessler and Hasslacher Chemical Company, of Niagara Falls, New York. He is being succeeded by Dr. W. E. Bradt, who for the past two and a half years has been on the teaching and research staff of the department of chemistry of the University of Cincinnati. Dr. Bradt received his bachelor's, master's and doctor's degrees from Indiana University.

DR. C. E. RESSER has been appointed curator of stratigraphic paleontology in the National Museum. He has been connected with the division of paleontology since 1915.

DR. N. ERNEST DORSEY, having completed his work as associate editor of "The International Critical Tables," has returned to the Bureau of Standards.

DR. WILLIAM B. MCDOUGALL, of the University of Illinois, has been appointed visiting professor of botany at the University of Southern California for the spring semester. He will fill the vacancy in the department of botany during the sabbatical leave of Professor Howard de Forest.

DR. HUGH CABOT, dean of the Medical School of the University of Michigan, has been relieved of his duties of that office and as director of the department of surgery. He continues as professor of surgery. The action has been taken owing to differences in questions of policy between Dr. Cabot and the faculty.

DR. CLARENCE C. LITTLE, formerly president of the University of Michigan, will act as director of the Mount Desert Island Biological Laboratory at Salisbury Cove for the coming six months during the absence of Dr. Herbert V. Neal, professor of zoology at Tufts College, who has gone on a scientific expedition to the Nile Valley. Dr. Little is at present director of the Dorr Station of the laboratory as well as director of the Roscoe B. Jackson Memorial Laboratory for Cancer Research.

PROFESSORS EDWIN O. JORDAN and William H. Taliaferro, of the department of hygiene and bacteriology of the University of Chicago, are spending the winter quarter at the School of Tropical Medicine, San Juan, Porto Rico. Professor Hans Smetana, of the University of Vienna, formerly of the Johns Hopkins Medical School and the Peking Union Medical College, has become assistant professor of pathology at the school, holding dual appointments in Columbia University and the University of Porto Rico. Professor George W. Bachman, formerly of the School of Hygiene and Public Health of the Johns Hopkins University, has accepted the post of associate professor of parasitology in Columbia University with duties at the School of Tropical Medicine.

PROFESSOR S. HATAI, of Tohoku Imperial University, Sendai, Japan, has extended an invitation to Professor Charles A. Kofoid, of the University of California, to be visiting professor of biology on the Rockefeller grant. Professor and Mrs. Kofoid sail on March 14 for a period of three months of lectures at the Biological Institute at Sendai and three months at the Marine Biological Station at Asamushi, at the northern end of Hondo, for graduate instruction and research.

MR. MATTHEW W. STIRLING, chief of the Bureau of American Ethnology, left Washington on January 13 for a cruise among the Ten Thousand Islands between Charlotte Harbor and Cape Sable, Florida, where he hopes to discover evidences of the former occupancy of this area by the Caloosa Indians. He is being aided by Mr. Lee Parish, of Tulsa, Oklahoma, whose 85-foot yacht, *Esperanza,* furnishes transportation. After his exploration in the Ten Thousand Islands region Mr. Stirling will excavate a large sand mound on the west coast of Florida, south of Tampa Bay.

C. B. PHILIP, associate entomologist for the Rocky Mountain Spotted Fever Laboratory of the U. S. Public Health Service at Hamilton, Montana, has returned from a tour of eighteen months in West Africa, as entomologist of the Yellow Fever Commission of the Rockefeller Foundation.

MR. M. F. BRAMLEY, of Cleveland, Ohio, has invited the Cleveland Museum of Natural History to send two men with him on a short entomological trip into Central America. The museum secured Dr. George P. Engelhardt, director of natural sciences in the Brooklyn Museum, to make this trip, along with their own entomologist, Mr. John C. Pallister. The party left Long Beach, California, on February 1, in Mr. Bramley's yacht, *The Peary.* This is the yacht used by MacMillan and Byrd on their Arctic expedition. Before going to Guatemala the party will spend a few days on the uninhabited island of Socorro.

DR. H. M. DADOURIAN, professor of mathematics and astronomy at Trinity College, Hartford, Connecticut, has leave of absence. With Mrs. Dadourian he expects to travel in the Near East and Europe, including Russia, returning at the end of September.

PROFESSOR F. A. MELTON, of the department of geology of the University of Oklahoma, will spend seven months from February to September in research work and study in southern and central Europe on the relationship of jointing to orogeny.

DR. PHILIP E. SMITH, professor of anatomy in the College of Physicians and Surgeons of Columbia University, will deliver the fifth Harvey Society lecture at the New York Academy of Medicine on February 20. His subject will be "Relations of the Activity of the Pituitary and Thyroid Glands."

DR. W. F. G. SWANN, director of the Bartol Research Foundation of the Franklin Institute, Philadelphia, recently delivered a lecture before the Royal Canadian Institute on "Philosophical Aspects of Modern Physics."

DR. OSCAR RIDDLE, of the Carnegie Institution, addressed the Commonwealth Club and the University Club of Chicago on January 24 and 25 on "Disclosing Mechanisms of Life."

PROFESSOR V. K. LA MER, of Columbia University, delivered two lectures before the department of chem-

istry of Brown University on January 10 entitled "Solutions of Electrolytes" and "An Extension of the Debye-Huckel Theory to High Valence Ions in Non-aqueous Solutions." Professor La Mer gave the same lectures before the departments of chemistry of the Universities of Wisconsin, Iowa, Nebraska, Kansas and Illinois and the Iowa State College at Ames.

DR. EDWARD FRANCIS, of the U. S. Public Health Service, will give two lectures under the Harrington Foundation at the University of Buffalo Medical School. The first will be on "Tularemia" on February 27, the second on "Undulant Fever" on February 28.

PROFESSOR ROBERT H. GAULT, of Northwestern University, read a paper before the Franklin Institute on the evening of January 30, entitled: "A Partial Analysis of the Effects of Dual Stimulation from Spoken Language."

PROFESSOR FRANCIS E. LLOYD, of McGill University, Montreal, delivered lectures on January 28 and 29 at Ann Arbor and Detroit. At the University of Michigan his subjects were "A Botanist's Tour in Africa" and "The Comparative Morphology and Physiology of the Bladders of Utricularia." In Detroit he spoke under the auspices of the United States Rubber Company on the "Culture of Plantation Rubber."

DR. FRIDTJOF NANSEN will lecture in London on "The Importance of Meteorological Research" on March 14 before a joint meeting at the Institution of Electrical Engineers of the Royal Aeronautical Society and the Royal Meteorological Society.

THE program of the non-biological science section of the Ohio State University Educational Conference on April 4, which will be presided over by Professor Wm. Lloyd Evans, is as follows: "Matter and Energy," Professor Alpheus W. Smith, the department of physics, the Ohio State University; "Symmetrical and Unsymmetrical Hydrogen," Professor Herrick L. Johnston, the department of chemistry, the Ohio State University; "An Interpretation of the New Mechanics," Dr. Saul Dushman, the research laboratory, the General Electric Company; "What are the Stars?" Professor H. T. Stetson, the department of astronomy, the Ohio Wesleyan University; "Some Applications of the New Mechanics to Physics and Chemistry," Dr. Saul Dushman; "What is Light?" Professor Arthur H. Compton, the department of physics, the University of Chicago.

PRESIDENT J. Y. SNIDER, of the American Association of Petroleum Geologists, in a letter addressed to the chairman of the section of geology and geography of the American Association for the Advancement of Science, extends a cordial invitation to all association members to attend the annual convention of the petroleum geologists to be held in New Orleans on March 20, 21 and 22, with headquarters at the Roosevelt Hotel. Those interested may obtain further information concerning this meeting by addressing J. P. D. Hull, Box 1852, Tulsa, Oklahoma.

DR. J. MURRAY LUCK, secretary of the Pacific Division of the American Association for the Advancement of Science, writes that at the thirteenth annual meeting of the division announcement was made that a prize of $100 would be awarded to the authors of the most important scientific contribution reported at the Berkeley meeting. The amount of the prize was increased by recent action of the executive committee to $150. Five papers were submitted to the committee on award as being worthy of consideration. Acting on recommendations received, the executive committee at its last meeting awarded the prize to the investigations on the isotopes of oxygen pursued by W. F. Giauque and H. L. Johnston, of the University of California, and H. D. Babcock, of the Mount Wilson Observatory. In this work convincing evidence was presented of the existence in small proportions of two isotopes of oxygen, masses seventeen and eighteen. The committee also acknowledged the unselfish services of Professor R. T. Birge, an international expert in matters relevant to the study of spectral lines, whose contributions were of invaluable assistance in bringing these investigations to a successful conclusion. Honorable mention was made of the contribution by E. B. Babcock and J. L. Collins, of the University of California, on the "Rate of Mutation in *Drosophila melanogaster* as Affected by Differences in Earth Radiation."

The University of Texas has recently come into possession of somewhat more than $800,000 bequeathed to it by the late W. J. McDonald, of Paris, Texas, for erecting, equipping and maintaining an astronomical observatory. The bequest was contested by relatives and the amount mentioned above is the result of a compromised suit. The university will proceed very carefully and slowly to plan for the future McDonald Astronomical Observatory, availing itself of the advice of the most competent astronomers.

H. R. FRY has given an eight-inch refracting equatorial telescope to the University of London Observatory in Mill Hill Park, which is administered by University College.

THE Research Corporation of New York has appropriated $15,000 for the work of the division of radiation and organisms of the Smithsonian Institution.

THE *Journal* of the American Medical Association reports that during the twelfth International Ophthalmological Congress, I. Van der Hoeve, president of the congress, and E. Marx, secretary, called a meeting at which the representatives of the following states and cities were present: Van der Hoeve, Leyden; Jitta and F. Wibaut, Amsterdam; Angelucci and V. Rossi, Naples; Lutrario, Rome; Emile de Grosz, Budapest; Auerbach, Moscow; F. Avizonis, Lithuania; F. H. Beaumont, New Zealand; Birch-Hirschfeld, Königsberg; Coppez, Brussels; A. Feigenbaum, Jerusalem; Gray Clegg, Manchester; Hamburg, Roumania; Hanke, Vienna; F. Humbert and Morax, Paris; Imre, Pécs; Lundsgaard, Copenhagen; Lussich-Matkovic, Zagreb; Marquez, Madrid; M. Nakashima and S. Myashita, Japan; J. Park Lewis, Buffalo; Walter R. Parker, Detroit; C. Pascheff, Sofia; Pavia, Buenos Aires; Soria, Barcelona; Symansky, Warsaw; Charles Weiss, United States; W. H. Wilder, Chicago; R. E. Wright, Madras; M. Zachert, Poland. The spread of trachoma and the necessity of the campaign against it were set forth in an exhaustive report by Lutrario and Jitta, delegates of the Comité d'hygiène of the League of Nations. It was resolved to form an international league, in accordance with the motion of Van der Hoeve. Dr. Emile de Grosz, professor of ophthalmology at the University of Budapest, was appointed president, and Dr. F. Wibaut, secretary. They were entrusted with the formation of a committee from among the delegates of the ophthalmic societies and leagues already existing. The president was authorized to communicate with the Comité d'hygiène of the League of Nations and the Rockefeller Foundation to endeavor to enlist their aid in the movement.

AN extensive trip by the department of geology of Princeton University is being planned for next summer, when the party will make a journey of more than 10,000 miles throughout the United States. Under the leadership of Professor Richard M. Field, chairman of the department, an investigation of the principal natural resources of the United States and their relation to national and international trade and transportation will be made. Besides a number of American scientific men, the party will include representatives from Germany, England, France and probably Italy and Japan. Among these will be Dr. Hans Schneiderhohn, professor of economic geology at the University of Freiberg, and Dr. Harbort, lecturer in the Technische Hochschule, Berlin.

A gift of $50,000 to the Massachusetts Horticultural Society, by its president, Albert C. Burrage, was announced at the inaugural meeting of the society. Mr. Burrage stipulates that this gift is to be divided into two funds, one amounting to $30,000 and the other to $20,000. The income from the former is to be used to purchase books for the society's library, already recognized as the largest and best horticultural library in the world. The income from the second fund is to be used for a gold cup to be awarded at the end of each year for the best exhibit which has been made at any of the society's exhibitions in the course of that year. Another gift of $2,500 came from W. N. Craig, of Weymouth, to be used for the improvement of lilies.

DISCUSSION

BACTERIUM GRANULOSIS AND TRACHOMA

Two expeditions to Indian schools in the southwest for the purpose of confirming Dr. Noguchi's work on trachoma[1] were undertaken in May and September, 1929. The invaluable cooperation of Drs. F. I. Proctor, P. Richards and J. F. Lane in this work is hereby gratefully acknowledged.

Three types of cases have been examined: first, cases of undoubted trachoma in pupils who had been in school for some time and had received treatment in the usual way; second, similar cases which had received no treatment, and finally, untreated cases of recent onset. Noguchi's original isolations of *Bacterium granulosis,* on his expedition to the southwest in 1926, were obtained with advanced untreated cases, and our isolations have so far been obtained either from untreated cases or cases which had had no treatment for several months previous to culturing. However, it is probable that treated cases can be made to yield *Bacterium granulosis* by repeated bacteriological examination.

Our first expedition yielded two strains of *Bacterium granulosis* from two advanced untreated cases. Negative results were obtained in five advanced cases which were undergoing treatment and in three cases in which the diagnosis was uncertain. The cultures were made on horse blood agar and on the Noguchi semisolid leptospira medium and examined on our return to New York, nine days after their preparation.

Our second expedition yielded four strains, two from advanced cases, with abundant scar tissue, one from a case in which the formation of scar tissue was just beginning and one from a recent case in which no scar tissue was present. These four cases had received no treatment for six months previous to culturing. Eleven advanced cases undergoing treatment yielded no *Bacterium granulosis,* nor did six cases in which the diagnosis of trachoma was doubtful.

[1] H. Noguchi, *J. Exper. Med.,* 48, Suppl. No. 2, 1928.

The strains of *Bacterium granulosis* obtained by us agreed culturally in all respects with the Noguchi bacterium. Two were tested individually in *Macacus rhesus,* both inducing the characteristic granular lesions of the conjunctiva. Three other strains isolated by us, when pooled and inoculated, produced in four of five monkeys typical experimental trachomatous lesions of marked severity, appearing first on the twelfth day and progressing to abundant development during the following six weeks.

E. B. TILDEN
J. R. TYLER

THE ROCKEFELLER INSTITUTE
FOR MEDICAL RESEARCH

THE PASSENGER PIGEON

IT is commonly believed that the passenger pigeon, so frequently seen through the middle west about fifty years ago, has ceased to exist on this continent. Several reports to the contrary have appeared from time to time, but it has usually been concluded by those in a position to know that these reports have concerned either the dove or the band-tailed pigeon from the west.

An article in the Detroit *News* for January 5 by Mr. Kendrick Kimball, staff correspondent of the *News,* reports some observations of interest made by Mr. Robert H. Wright, of Munissing, Michigan (Upper Peninsula), and by Dr. Samuel R. Landes, of Traverse City. The former is said to have observed a pair of passenger pigeons on highway M-28, about sixteen miles from Munissing, on June 10, 1929. They were in the road and he approached within ten feet of them—sufficiently close for him and his family to note the sheen on the neck and the red eyes. Dr. Landes, driving from Florida, observed a flock of about fifteen between Indianapolis and Kokomo. Both these men were said to be quite familiar with this species, having hunted and plucked hundreds of the birds in Michigan and Missouri, respectively, in the late seventies.

The article by Mr. Kimball has served as the incentive for me to report an observation made in the wilds of the North Peninsula of Michigan during the second week of September last.

An old friend, Mr. Foard, of Marquette, and myself were finishing a day devoted to running out lines of a tract of some six hundred acres of my summer camp on the north shore of Lake Huron, near Beaver Tail Point, about twelve miles east of Les Cheneaux. The country is wild, practically uninhabited, and lies mostly in "burnings" interspersed with patches of fresh growth of spruce, poplar and some old timber.

About six-thirty in the evening we were still some five miles from camp, pushing a line through the scrub and attempting to locate a trail. We had just paused to get our direction when Mr. Foard pointed to the top of a tall, dead white birch and said, "There's a passenger pigeon; I have seen several the last few years." I had to turn in order to look, and then the bird was already in flight, perhaps a hundred feet away. All that I could see was that it had the shape of a pigeon and a somewhat pointed tail. Mr. Foard had the better view and had seen the bird several moments before he spoke. While I can not therefore verify this report, I have great faith in Mr. Foard's powers of observation. His entire life has been spent in the woods of northern Michigan, as land agent and trespass agent for the government. Few in the state, I dare say, know more about the denizens of the Michigan woods than he. The deer, the bear, the timber-wolf and the birds have been his companions for forty years, and he knows them and their ways. As he told me when we continued our way homeward, discussing the strange disappearance of the passenger pigeon, he had often hunted the birds in the years around 1878 in this same country and knew them too well to be mistaken. Since their long disappearance he had seen none until quite recent times.

It seems to me that these three observations by Michigan men, all of whom knew the passenger pigeon in the old days, serve seriously to raise the question that has been propounded by Mr. Kimball in his newspaper article: "Has the passenger pigeon come back?" The northern peninsula of Michigan was once a famous nesting ground for this species, and it is hoped that the residents of this delightful region will continue to make special effort to recognize and report this assumedly extinct species whose sudden disappearance constitutes one of the greatest recent biological tragedies of the American continent.

PHILIP HADLEY

UNIVERSITY OF MICHIGAN

SEVERE HAIL INJURY TO TREES AND SHRUBS

ONE evening last summer, as the writer was returning home from a trip through southern Illinois, his attention was attracted by the peculiar appearance presented by the trees growing in a cemetery at the north edge of Marion, the county-seat of Williamson County, Illinois. As seen from the road, the foliage on practically all these trees appeared to be in crowded, rosette-like bunches on the larger limbs, while the small limbs were conspicuously dead and barren. It was almost dusk at the time, and as there were many miles yet to be driven, there was little inclination to tarry long enough to make a detailed examination of the trees. Later, however, on another trip to the locality, made on October 13, in company with Dr. Benjamin Koehler, crop pathologist at the

University of Illinois, a close examination was made of these trees and of the shrubs growing with them.

It was at once apparent that early in the season the trees had been stripped of their young developing foliage and small twigs and that the larger twigs and small limbs had been killed by some excessive injury. These stood out in the picture dead and barren against the greenness of a second growth of foliage and young twigs which, on all the trees, had since grown out very abundantly from a multitude of adventitious buds. The general aspect of these trees was such as to remind one of the war pictures of shrapnel-swept battlefields, where broken trees stand here and there denuded of their leaves after the bombardment. Truly these trees had been bombarded, for a terrific storm of hail had swept this area in the previous spring.

The numerous and markedly compacted bunches of new foliage, which from a distance appeared so much like rosettes, were generally abundant in all parts of the trees, in many cases even to the extent of growing out densely up and down the trunks, as if the trees, in a desperate effort to retain their hold on life, had made use of all their resources to reclothe themselves with leaves.

A close examination of the twigs and limbs of the trees, which were mostly maples and oaks, revealed severe hailstone lesions on the upper sides. These were frequently an inch or more long and often coalesced to form long compound lesions which, in many cases, resulted in a virtual girdling of the part.

From the many smaller branches and twigs which had been killed, in some cases for several season's growth back, by the excessive loss of cambium the bark was still peeling where it had not previously been stripped off by the hailstones.

The lesions on the still living twigs and small limbs were healing over very nicely with no sign of infection of any sort. The approaching layers of new tissue from the two sides of the long narrow lesions had, in the case of the smaller ones, already met, and in other instances, where the injury had extended half to three quarters of the circumference of the twig or limb, the healing process, although progressing very nicely, was not yet finished. The condition presented an excellent example of wound healing such as one is not often likely to meet with under ordinary circumstances.

On small ornamental junipers and Japanese barberry in the cemetery the foliage appeared normal, but a close examination of the twigs of these shrubs revealed severe hailstone lesions which, as in the case of the trees, were healing over very nicely.

Upon inquiry, the writer was informed by a local resident that on the first of May, 1929, a most severe hailstorm had occurred as a general disturbance over the entire locality but that the fall of hail had been exceedingly severe over the cemetery where, judging from the damage done, it had occurred in a path about two or three hundred yards wide from north to south. On neither side of this path did the vegetation show any such evidence of severe injury as was inflicted on the cemetery trees.

The hailstones were reported to be very large, almost the size of small hen's eggs. Automobile tops which were exposed to the storm were reduced to sieves by the bombardment. Hailstones of this size have occurred in Illinois before, although they have not been known by the writer previously to inflict such severe damage to trees. In the spring of 1927 particularly severe hailfall of very large stones occurred at Urbana and in the East St. Louis area as well as in St. Louis, Missouri, when great damage was done to houseroofs, greenhouses and automobile tops. At that time the Missouri Botanical Garden suffered severe damage to its greenhouses and to the very valuable plants contained in them.

GILBERT L. STOUT
ILLINOIS STATE NATURAL HISTORY SURVEY

MAN-MADE EARTHQUAKES

ON October 18, 1929, between midnight and sunrise, slight earth tremors were felt at Big Springs and nearby towns in Texas. People were very generally awakened by the rattling of dishes and windows. One person at Big Springs states that he was awakened about 3 A. M. by what he thought at the time was an automobile hitting the house in which he was sleeping. Another says he was awakened at about this time thinking some one was rattling his door. At one place south of Stanton a lamp chimney was broken, and at another the glass in a door was fractured. Five or more distinct tremors were felt of which the last, occurring about daylight, was said by some to be the strongest. On the following night similar tremors were felt although apparently by fewer persons. Some who had lived in earthquake regions were positive that the tremors were earthquakes.

Inquiry made in the region affected indicated that the tremors were caused by explosions of dynamite made in connection with the use of the seismograph in geologic explorations. The company making these explorations very kindly supplied a record of seismograph shooting on October 17, 18 and 19, and this note is made to record the distance that such earth tremors from seismograph shooting may be transmitted under favorable conditions in sufficient force to attract general attention.

The record of the seismograph work indicated that on the forenoon of October 17, two shots were fired on Section 15, Block 36, T.4 S. At midnight shooting was resumed at this locality and eight shots were fired between 12:20 and 8:15 A. M., October 18. Charges of dynamite were used in the following order: 50, 300, 300, 900, 1,500 pounds, and 50, 300, 300, 900 and 1,800 pounds. The 1,500-pound shot fired at 3 A. M. on October 18 made the tremor which caused one person at Big Springs to think that an automobile had bumped his house and another to think that some one was rattling the door; the 1,800-pound shot fired at 6:15 A. M. coincides with the reports as to the strongest tremor felt. The shooting on the following night, October 18–19, was as follows. On Section 39, Block 36, T.3 S. from 9:25 to 11:30 P. M.: 50, 300, 300, 900, 1,500 pounds; from 12:50 to 3 A. M.: 50, 300, 300, 900, 1,500 pounds, and on Section 31, Block 35, T.3 S. from 3:50 to 5:55 A. M.: 50, 300, 300, 900, 1,500 pounds.

The 1,500-pound shot at 3 A. M. on October 19 seems to have been less generally felt than was the same charge at the same time on the previous night. All the shots were similarly placed, being covered by about five feet of earth. The temperature at 3 A. M. on October 18 was 14° C.; the wind velocity, 11.2; the direction of the wind, from 10 degrees east of north; the sky cloudy. On the next night the temperature at 3 A. M. was 18° C.; the wind velocity, 8; the wind direction, from 10 degrees east of south; the sky cloudy. The ground conditions at the locality of the shot which are not recorded in detail may account for the seemingly different intensity of the tremors. One or two observers at Knott report that the tremors of the second night were greater than those of the first. However, for the region as a whole those of the second night were not so generally felt or reported upon.

The shots of October 17 and 18 were made fourteen miles west of Garden City in Glasscock County. The tremors were felt at Sterling City forty-four miles east; at Big Springs thirty-two miles northeast; at Knott forty miles north; at Stanton twenty miles north, and at Midland twenty-one miles northwest. Inquiry among farmers and ranchmen indicated that even beyond the towns mentioned the tremors were more or less distinctly felt. Apparently the tremors were readily detected within a distance of forty or fifty miles east and north from the locality at which the dynamite was discharged, this being true at least for the larger shots of 1,500 and 1,800 pounds. The distance the tremors were felt west and south is less well known from lack of records. The surface formation at the locality where the dynamite was exploded and of much, but not all, of the country affected is Cretaceous limestone. Where the limestone is absent the surface formations consist of either Cretaceous sands or Triassic or Permian red sands and clays. The limestone is apparently an effective medium for the transfer of earth tremors.

E. H. SELLARDS

UNIVERSITY OF TEXAS

IN AID OF AMERICAN MEDICAL BIOGRAPHY

DR. I. FISCHER, of Vienna, is engaged in the preparation of supplementary volumes to the second edition of Hirsch's "Biographisches Lexikon der hervorragenden Aertze aller Zeiten und Völker" (Berlin and Wien, 1884–6), which is the only international dictionary of medical biography in existence. The supplement will contain biographies of recent and living physicians, and Dr. Fischer is particularly anxious to obtain American material, the conditions for inclusion being original laboratory and clinical work, important discoveries and inventions, publications, editorial work and such like. Any help which may be given him by the donation of "Who's Who," volumes of biographical sketches, clippings of important obituaries from medical periodicals, etc., will be much appreciated. Such material may be transmitted directly to Dr. Fischer himself (address: Biberstrasse 15, Vienna, I, Austria).

F. H. GARRISON

ARMY MEDICAL MUSEUM

SCIENTIFIC BOOKS

Outlines of Biochemistry (The Organic Chemistry and the Physicochemical Reactions of Biologically Important Compounds and Systems.) By Ross AIKEN GORTNER. John Wiley and Sons, New York, 1929.

THE success of the application of physics and chemistry to biology depends upon two conditions: first, the ability of the biologist to master physics and chemistry and yet remain a biologist, and second, the willingness of the physicist and the chemist to cooperate sympathetically with the biologist. Professor Ross Aiken Gortner is an outstanding example of the fulfilment of the first condition. In reading his "Outlines of Biochemistry," it would be difficult to say whether Professor Gortner is biologist or

chemist. He speaks to both in their own language, an attribute which few possess.

The second condition, that of the willingness of the physicist and the chemist to join hands with the biologist, is one which is differently viewed by people on both sides. There is the biologist who sees only futility in the attempt to solve vital problems in terms of physics and chemistry. Often have I received the injunction from biologists to remain a pure physiologist and not venture too far into the fields of physics and chemistry; and often have physicists and chemists said to me, "Good Lord, you talk of the structure of protoplasm, a *living* thing of tremendous complexity, and I do not yet know the structure of gelatin," or, "Why speak of the plasticity of protoplasm when we do not know the full meaning of this property as applied to paint and starch?" Fortunately, not all of us are of this mind. A delightful example of the opposite view-point came to my attention this past summer, and it came soon after I had heard the plaintive cry of a biologist who all his life has attempted to apply physical and chemical laws to biological processes, and now, with his work over, looks back on it as a failure. But not so Geheimrat Fritz Haber. We had just heard a lecture at the Kaiser Wilhelm Institute on sexuability in the lower organisms, and we waited, hoped, for some suggestion of the cause of the phenomenon, just one thought beyond the experiments, but none came. (I believe it was Professor Gortner himself who once said, "Science consists not only in the assembling of facts but in the marshaling and interpretation of these facts.") Fritz Haber then arose and told how he had long looked forward to the time when biologists would search for the cause of such characteristics of organisms as sex intergrades, in the specific nature of the proteins, the polarity of the proteins, of the respective individuals. Now, the task which Haber has given the biologist is a big one; how are we to isolate these proteins, and how then determine those specific characters which are responsible. But Haber's point of view is as far-sighted as it is courageous; he has a vision into the future, and it is such a vision, based on sound experimentation, which Professor Gortner gives us in his "Outlines of Biochemistry."

The book is of nearly eight hundred pages, well illustrated and excellently printed. There are thirty-five chapters separated into seven parts as follows: I. The Colloid State of Matter; II. Proteins; III. Carbohydrates and Allied Compounds; IV. The Tannins; V. Plant Pigments; VI. Fats, Lipides and Essential Oils; VII. The Biocatalysts.

Students often come to me and ask what book they should get in order to familiarize themselves with colloid chemistry, and until now I have had to tell them that there is no book which meets their specific requirements. Professor Gortner has supplied this need in Part I of his volume.

One of the valuable features of the book is brought out in the first part. Professor Gortner again and again calls attention to the many applications of a fact or an experiment in related fields; thus, in defining hysteresis he points out that although the word refers to a lag of some sort when used both in biochemistry and in engineering practice, yet the term is not the same in the two cases.

The last section of Chapter VI is on adsorption. This subject, which has played so important a part in Professor Gortner's own work, is treated fully and well. The theory of adsorption is first discussed, and then problems and applications given. There is included a definition of "negative" adsorption. Here is a term I have never liked. I react to it as I do to the popular conception of suction as a "negative" pressure, and to the definition of osmosis as "diffusion from a region of low concentration to one of high concentration." The difficulty in all these cases is, that while the thing we are thinking about is apparently negative, somewhere else in the system there is a positive force which is the cause of the supposed negative event; thus, in "negative" adsorption the solvent is positively adsorbed.

In the chapter on gels there is a useful discussion of imbibition and the rôle of water in the living organism—this might well have been made a chapter by itself. There is included an account of one of Professor Gortner's contributions to agriculture, on the nature of winter hardiness in wheat. In this connection the important fact is stressed that the biochemistry of both plant and animal cells has much in common, for the same factors which enable plant cells to live through the winter are likewise factors in the winter hardiness of insects. Work of this character, in what is usually referred to as "applied" botany, is an excellent example of the close interrelation of theoretical science and practical needs, and of the application of the one to the other. Indeed, Professor Gortner repeatedly shows, both in his work and in his book, how true is Pasteur's exclamation, *"Non, mille fois non, il n'existe pas une catégorie de sciences aux quelles on puisse donner le nom de sciences appliquées. Il y a la science et les applications de la science, liées entre elles comme le fruit et l'arbre, qui l'a porté."*

Part II, on proteins, takes the subject up systematically with interesting chapters on the structure of proteins, and the biological (immunological) reaction of proteins. The remark of Fritz Haber (quoted

above) is substantiated in Professor Gortner's treatment of the proteins and in the introductory quotation from Emil Fischer: *"Da die Proteinstoffe bei allen chemischen Prozessen im lebenden Organismus auf die eine oder andere Weise beteiligt sind, so darf man von der Aufklärung ihrer Struktur und ihrer Metamorphosen die wichtigsten Aufschlüsse für die biologische Chemie erwarten."* Pauli has also laid emphasis on the proteins as the ultimate seat of vital phenomena in saying that "they alone display the specific properties of living matter."

In discussing certain phases of the controversy precipitated by Jacques Loeb, on the relative importance of the H ion, as compared with other ions, on adsorption, degree of peptization and like phenomena, Professor Gortner states that "we have here a lyotropic series . . . the degree of peptization can be due only to a specific influence of the anions, . . . proteins must be considered not alone as complex organic compounds but likewise as colloid micelles, subject to all the varied reactions of a lyophilic system." It is interesting to see the pendulum swing back again to Nägeli's micellae and Hofmeister's series.

The biologist will be particularly interested in the chapter on "The Biological Reactions of the Proteins" since this includes a consideration of phylogenetic relationships as determined by serological reactions.

Part III is on the carbohydrates. It includes sections on photosynthesis and fermentation. Here, as elsewhere, the book shows its special value to the general botanist and zoologist, and again in Part V on plant pigments where the relationship between blood pigments and chlorophyll and the chemistry of color inheritance are considered.

Five chapters on fats, lipides, oils, vitamins (prepared by L. S. Palmer) and enzymes complete the book proper. Eight pages of general reference and two indexes are appended.

In addition to its scientific value—and this can not be overestimated—there is a human, or philosophic side to Professor Gortner's book which is especially fine. In discussing histamine he says, "Toad skin, according to Pliny, was a medicine. Abel showed that it contained a powerful drug. This is an additional instance where one of the old folk remedies was found to be not so foolish as it appeared to be." In many similar remarks one realizes that the author is a man who, though taking his work seriously, still appreciates the uncertainty and the humor in science.

WILLIAM SEIFRIZ
UNIVERSITY OF PENNSYLVANIA

REPORTS

THE CANCER RESEARCH FUND OF THE UNIVERSITY OF PENNSYLVANIA

THE Cancer Research Fund of the graduate school of medicine of the University of Pennsylvania has received a gift of $210,000 from a philanthropist who does not desire to have his name revealed. It has been realized for some time that progress in cancer research work depends upon the association of the physician, the biologist and the chemist and upon the close cooperation of related clinics and laboratories. And now, as the result of the generous support which has just been given by a philanthropist from whom the Cancer Research Fund will receive $70,000 annually over a period of three years, the school has been placed in a position to carry on the cancer research work along these broad cooperative lines to an extent which heretofore was impossible.

One joint research project has been undertaken in association with the Bartol Research Foundation of the Franklin Institute, which is one of the great physical research institutes of the country, while research work has also been arranged for in association with the American Oncologic Hospital, a cancer hospital at Thirty-third Street and Powelton Avenue.

Plans are being developed for the equipment of a new clinic for modern diagnosis and treatment of tumors in the American Oncologic Hospital, and Dr. George M. Dorrance, professor of maxillo-facial surgery in the school of dentistry of the university, has been appointed chief of the clinic. Dr. Dorrance is assembling a group of specialists of the highest type to serve on the clinic staff, and the American Oncologic Hospital will be developed gradually into an anti-cancer center of the most complete character. Central laboratories of the Cancer Research Fund have been established at 133 S. Thirty-sixth Street, and here a complete tissue culture department is under the direction of Dr. Clarence E. McClung, professor of zoology at the University of Pennsylvania, and Professor David H. Tennent, of the department of zoology of Bryn Mawr College. Professor J. P. M. Volgelaar, of the University of Leyden, Holland, and Dr. Raymond C. Parker, the latter of whom recently was connected with the Kaiser Wilhelm Institute of Biology in Berlin, also have been added to the staff of the tissue culture department, and eight other research workers will be maintained in this department.

The central laboratories also have an experimental animal cancer department under the immediate direction of Dr. Shigemitsu Itami, who has had ten years' experience at the Crocker Cancer Institute of New York and four years with the British Empire Cancer Campaign in London, and who is assisted by a chemist and zoologist. Research work in radiation and other physical aspects of cancer is being carried on in the central laboratories in collaboration with the Bartol Institute, and for this purpose the Cancer Research Fund is maintaining two physicists, two chemists and a scientific secretary.

At the Philadelphia General Hospital the Cancer Research Fund has had for some time a research laboratory for the study of biochemistry of the blood before and after radiation of patients, and for the study of the hematology and immunology of cancer. This laboratory works in conjunction with the cancer clinic of the Philadelphia General Hospital, and has a staff of three chemists, one immunologist, one physician and one voluntary worker.

In addition to maintaining the laboratories mentioned, the Cancer Research Fund has made a number of grants for investigation to be carried on by research workers in collaboration with the work of the fund. These are as follows: A study of the metabolism of cells, by Professor W. D. Bancroft, of Cornell University; a study of the fixation of pathological material, by Dr. Henry J. Fry, of New York University; a study of the sensitization of cells to fluorescent substances and specific wave-lengths of X-rays, by Professor Eric Ponder, of New York University, and a study of the quantitative spectrographic estimation of minute quantities of substances in biological material, by Dr. Andrew Dingwall, of the department of chemistry, Columbia University.

In connection with the last-named study the fund has established a fellowship under the name of the Dr. J. Packard Laird Fellowship for Cancer Research; this fellowship is held at present by R. J. Crosen, of Columbia University. Another fellowship to be known as the Dr. John G. Clark Fellowship in Cancer Research also is being established.

The director of the cancer research is Dr. Ellice McDonald, assistant professor of gynecology in the graduate school of medicine of the university, and the Cancer Research Fund is controlled by an executive committee consisting of Dr. McDonald, Dr. George H. Meeker, dean of the graduate school of medicine, and Professor W. F. G. Swann, director of the Bartol Research Foundation and special lecturer on electrical engineering at the university.

In addition there is the following board of scientific directors of the research: Dr. McDonald, *chairman;* Dr. Meeker; Professor Swann; Dr. John A. Kolmer, professor of pathology and bacteriology in the graduate school of medicine; Dr. Eugene L. Opie, professor of pathology in the graduate school of medicine; Dr. Martin Kilpatrick, assistant professor of chemistry, University of Pennsylvania, and Dr. Clarence E. McClung, professor of zoology at the university. The board of scientific directors has recently established a publications committee with Dr. Opie as chairman.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A DIET FOR STOCK RATS

DURING the past seven years the stock rats of this laboratory have been reared on a diet which has given uniformly satisfactory results from the standpoint of growth and fertility and which has the further advantage of being obtainable ready-mixed at a very reasonable cost.

This ration was originally developed by Maynard, Norris and Krauss[1] as a feed for young calves. It was later tried out with rats with such good results that it was adopted as our stock diet. The formula[2] for this diet as now used is as follows:

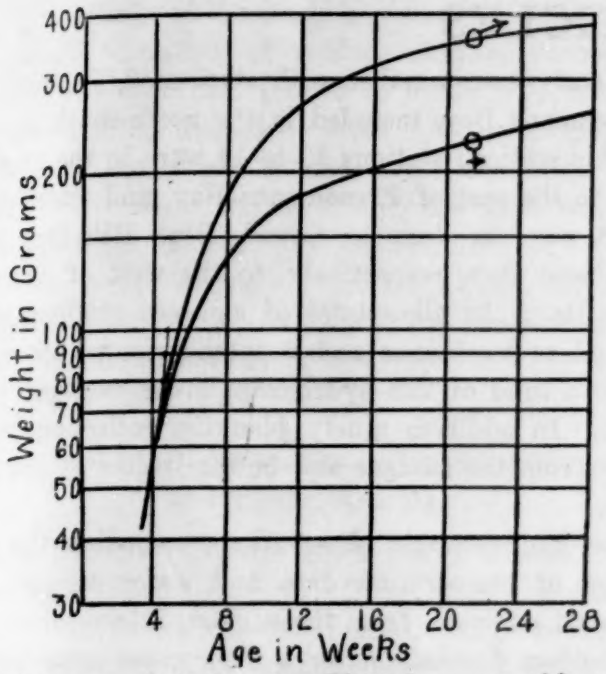
300 lbs.	Linseed oil meal
200 "	Ground malted barley
440 "	Wheat red dog flour
300 "	Dried skim milk
300 lbs.	Oat flour
400 "	Yellow corn meal
20 "	Steam bone meal
20 "	Ground limestone
20 "	Salt

Originally the formula contained some soluble blood flour, a special product which later became difficult to obtain and which was therefore replaced by the dried skim milk. The diet is used as the sole ration, with the exception that cod-liver oil is fed twice a week, mixed in as 3 per cent. of the day's food. No green food of any kind is supplied.

The mean growth curves for our rat colony, plotted upon a semi-logarithmic scale, are shown in the chart. The animals providing the data for these curves were weaned at twenty-three days of age. The foundation rats for our colony were obtained from the stock of Osborne and Mendel. Since 1919 no animals from any outside source have been introduced.

[1] Cornell Agr. Expt. Sta. Bull. 439, 1925.

[2] This formula is marketed as a ready-mixed feed under the name G. L. F. Calf Meal by the Cooperative G. L. F. Exchange, Buffalo, N. Y.



Growth curves of stock rats. Plotted on a semi-logarithmic scale diagram.

The efficiency of this diet for reproduction is indicated by the following data. In August, 1929, all the females of the stock colony which were over one hundred days of age and less than one year were mated. Of this group fifty-two were young females which had never before been mated and twenty-eight were mature animals which had produced one or more litters. A male was placed with each three females and left with them for a period of five days. All the males were young and untested as regards fertility. From these matings fifty-two of the females, 65 per cent., produced litters. In view of the fact that most of the animals used were young and untested and that the mating period was limited to five days, we feel that the above records indicate a high degree of fertility. The average number per litter from the above matings was 7.3. Each mother was given six young to rear with the exception of thirteen mothers which were given five only. Thus the fifty-two mothers were allowed a total of 299 young. Of this number over 90 per cent. were reared and successfully weaned at twenty-three days of age.

Recently this stock diet has been tried out with rats in three other laboratories and uniformly satisfactory results have been reported to us. During the past year we have used it as a stock diet for our mouse colony with excellent results. Since this ration is mixed in hundreds of tons for marketing as a calf meal, it is available at a price which is very much lower than the cost of the ingredients and labor involved in the preparation of stock diets in small quantities in experimental laboratories.

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CORNELL UNIVERSITY

AN IMPROVED CALOMEL ELECTRODE VESSEL

IN much previous work in this laboratory the salt bridge connecting cell and electrode has been a cotton string wet with KCl solution of the same concentration as that used in the electrode. This string led into a beaker into which also dipped a calomel electrode tip of a conventional type. When six or more electrodes were used on a single cell, there was difficulty in getting the electrode tips with their attendant beakers close enough together. Even though the beakers stood on a paraffined bench, the slight contamination of the paraffin surface with moisture and spilt KCl gave cause to doubt the perfect insulation of the electrodes from the ground and from each other. An electrode vessel of the type shown in Fig. 1 was therefore used. The cup into which the

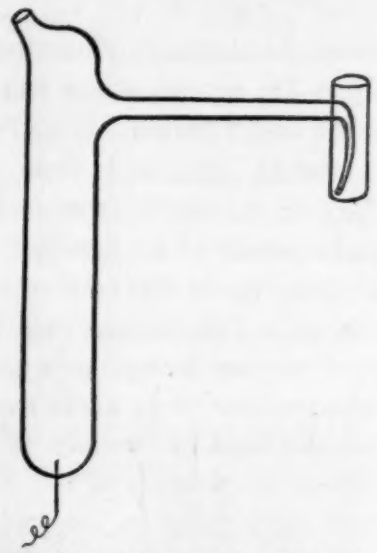

FIG. 1. Calomel electrode vessel with attached cup for string salt bridge.

string dips is blown onto the end of the electrode arm, which is long enough to permit placing the cups as close together as desired. The electrode arm enters the side of the cup, so that the entire surface of the electrode may be wiped with paraffin oil without oiling the solution in the cup. The arm is drawn to a capillary and extends to the bottom of the cup to prevent much mixing of the solution in the cup with that in the electrode. At the end of the day's work the cup is emptied with a pipette, more solution from the electrode vessel is flushed through and a rubber stopper is inserted to prevent evaporation of water and consequent creeping of KCl. The electrode vessel is held in a grounded metal clamp from which it is sufficiently insulated by the glass, oiled with paraffin. This electrode vessel has been found a great time-saver.

SAMUEL E. HILL

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SPECIAL ARTICLES

OCEANOGRAPHIC INVESTIGATIONS IN THE INSHORE WATERS OF THE GULF OF MAINE

IN the summer of 1929 a study of the biological, chemical and physical conditions of the inshore waters of the Gulf of Maine, in the vicinity of Mt. Desert Island, Maine, was undertaken by the Buffalo Museum of Science in cooperation with the Mt. Desert Island Biological Laboratory and Brown University.

The general plan of the work was divided and carried out by the various investigators as follows: Biological oceanology by Charles J. Fish; chemistry by Norris W. Rakestraw, and physical oceanography by H. R. Seiwell. Mrs. Charles J. Fish and Mrs. H. R. Seiwell assisted in the laboratory examinations of field data.

The field observations extended over the period from July 8 to August 21; investigations were made from Dyers Bay on the east (station 14) to Penobscot Bay on the west (station 18), and from the head of Frenchmans Bay on the north (station 2) to a point about fifteen miles south of its mouth (station 11).

The most intensive study was conducted at the eight stations established in Frenchmans Bay, four complete and two partial cruises being made in this region. Two sets of observations were made over the section extending from the head of the bay (station 2) to a point about fifteen miles south of the mouth (station 11). The small bays lying to the east were visited only once as also was Sommes Sound, and two sets of observations were taken each in Blue Hill and Penobscot Bay.

The arrangement of stations in the region was (Fig. 1): stations 1 to 8 were in Frenchmans Bay; stations 9 to 11 were to the south of Frenchmans Bay and were, together with stations 2, 7 and 8 of Frenchmans Bay, included in the north-south hydrographic section; stations 12 to 14 were in the smaller bays to the east of Frenchmans Bay, and stations 16 to 18 were in Sommes Sound, Blue Hill Bay and Penobscot Bay, respectively, to the west of Frenchmans Bay. In all a total of eighteen stations were worked at least once and usually twice or more, at which a total of 228 hydrographic observations were made. In addition ninety plankton collections were made from the surface and bottom water of the stations.

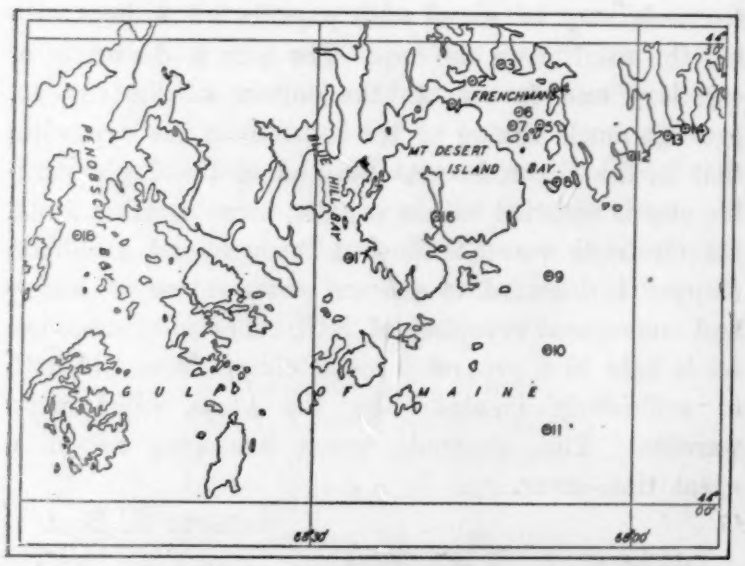

FIG. 1. Region of hydrographical investigations in the vicinity of Mt. Desert Island, Maine, July and August, 1929. Hydrographic stations marked thus—⊙

The hydrographic observations embodied the collection of temperature data and water samples for chemical analyses, from the surface, intermediate and the bottom depths. After a little experimentation it was found desirable to make all vertical hydrographical observations whenever possible at depths of 0, 5, 10, 25 meters and the bottom. All water samples were collected with Greene-Bigelow reversing water bottles to which were attached Negretti and Zambra deep-sea reversing thermometers. Six-millimeter wire was wound on a hand reel and passed over a Bergen Nautik meter wheel for measuring the depth of observation. All observations were conducted after the methods adopted by deep-sea oceanographers. Field headquarters were established during the summer at the Mt. Desert Biological Laboratory at Salisbury Cove, Mt. Desert, Maine. A small open boat, about eighteen feet in length, the property of the laboratory, was used for field operations.

The following is a very brief summary of the results of the physical observations made while the survey was in progress. The temperature is expressed in degrees centigrade and the salinity in grams of total salt per liter of sea water, represented by 0/00.

HYDROGRAPHICAL OBSERVATIONS IN FRENCHMANS BAY

Temperature.—Depth at stations, 12 to 86 meters. The average surface temperature ranged from 12.6° on July 8 to a maximum of 14.1° on July 31 and then to 12.0° on August 6; the average of all surface temperature observations was 12.8°. The maximum variation of surface temperature during the season was from 10.3° to 15.4° and the lowest temperature, 7.0°, was recorded at the bottom near the center of the bay. The water at the head was warmer than that at equal levels at the mouth of the bay. The average temperature of the whole bay from surface to bottom during the period of observation was 10.5°.

Salinity.—The average surface salinity increased from 31.73 0/00 on July 8 to 32.09 0/00 on July 31 and decreased to 32.04 0/00 on August 6; the average

surface salinity of the whole bay was 31.91 0/00. The maximum variation of surface salinity during the season was from 31.51 0/00 to 32.25 0/00, and the highest salinity recorded, 32.45 0/00, was near the bottom at the mouth of the bay. The least saline water occurred at the head and the most saline at the mouth of the bay, but the variation at any one time was never of very great magnitude, the maximum recorded being 0.54 0/00. The average salinity from surface to bottom of the whole bay during the season was 32.04 0/00.

Hydrographical Observations at Stations West of Frenchmans Bay

Blue Hill Bay (station 17) depth 80 meters. Temperature ranged, July 26, from 14.06° surface to 10.06° bottom, and on August 20, 13.81° surface to 11.10° bottom. The temperature below twenty-five meters was one to two degrees higher than in Frenchmans Bay. Salinity ranged, July 26, 31.60 0/00 surface to 32.12 0/00 bottom, and on August 20, 31.83 0/00 surface to 32.34 0/00 bottom. The surface salinity was about 0.50 0/00 lower than in Frenchmans Bay.

Penobscot Bay (station 18) depth 58 meters. Temperature ranged, July 27, 14.98° surface to 8.70° bottom, and on August 21, 13.07° surface to 10.0° bottom. The temperature below twenty-five meters was a little higher than in Frenchmans Bay. Salinity ranged, July 27, 27.72 0/00 surface to 31.98 0/00 bottom, and on August 21, 30.30 0/00 surface to 32.09 0/00 bottom. The surface salinity was the lowest observed in the region investigated and was 2.00 0/00 to 4.00 0/00 lower than in Frenchmans Bay.

Sommes Sound (station 16) depth 27 meters. Temperature ranged, July 26, 14.95° surface to 11.0° bottom, the temperature below five meters was a little higher than in Frenchmans Bay. Salinity ranged, 31.91 0/00 surface to 32.05 0/00 bottom, about the same as at the upper shallow end of Frenchmans Bay.

Hydrographical Observations at Stations East of Frenchmans Bay

Prospect Harbor (station 12) depth 16 meters. Temperature ranged, July 23, 12.59° surface to 9.11° bottom; salinity ranged 32.09 0/00 surface to 32.08 0/00 bottom. Both temperature and salinity were about the same as at the mouth of Frenchmans Bay.

Gouldsboro Bay (station 13) depth 18 meters. Temperature ranged, July 23, 12.86° surface to 10.40° bottom, the whole column of water was 0.3 to 1.3 degrees warmer than Prospect Harbor. Salinity ranged, 32.09 0/00 surface to 32.14 0/00 bottom, about the same as previous station.

Dyers Bay (station 14) depth 9 meters. Temperature ranged, July 23, 10.72° surface to 10.01° bottom; salinity 32.10 0/00 surface to 32.14 0/00 bottom. Temperature and salinity were about the same as at the mouth of Frenchmans Bay.

Hydrographical Observations at Stations South of Frenchmans Bay

On July 16 the surface temperature at station 10 was 10.42°, or about 1 degree lower than the temperature at the mouth of Frenchmans Bay lying ten miles to the north, while the temperature at 50 meters, 7.4°, was practically the same. On August 3, the surface temperature at station 11 was 10.0°, or 1.2 degrees lower than at the mouth of Frenchmans Bay lying fifteen miles to the north, while the temperature at 50 meters 8.1° was about 0.2° lower.

The surface salinity at station 10 on the former date was 32.10 0/00, or 0.06 0/00 lower, and the 50-meter salinity 32.52 0/00, or 0.18 0/00 higher than at the mouth of Frenchmans Bay. On the latter date the surface salinity at station 11 was 32.54 0/00, or 0.34 0/00 higher, and the 50-meter salinity 32.70 0/00, or 0.35 0/00 higher than at the mouth of the bay.

H. R. Seiwell,
Curator, Invertebrates and Oceanography
Buffalo Museum of Science

THE RELATION OF THE OXYGEN TENSION IN THE EXTERNAL RESPIRATORY MEDIUM TO THE OXYGEN CONSUMPTION OF FISHES

Recently two American articles by Powers and Shipe[1] and F. G. Hall[2] have had the above relation as their subject, and in both cases the conclusion was drawn that the fish's oxygen consumption is proportional to the oxygen tension of the environment, even when a restricted range of the latter within the normal concentration of natural waters is considered. It is perhaps unnecessary to emphasize the theoretical and methodological importance of such a conclusion. Unfortunately it appears for a number of reasons quite certain that this conclusion can not be accepted without qualification.

Before demonstrating that this conclusion is unwarranted at the present time, let me hasten to say that, as would be expected, the respiration of fishes is undoubtedly depressed at low levels of oxygen tension of the environmental water. Such a depression of respiration as the oxygen tension approaches the asphyxial threshold by no means implies a depression of oxygen consumption brought about by deviations

[1] E. B. Powers and L. M. Shipe, *Publ. Puget Sound Biol. Sta.*, 5: 365–372, 1928.
[2] F. G. Hall, *Am. J. Physiol.*, 88(2): 212–218, 1929.

within the normal range of oxygen concentration in the environment. For sea water we may consider this normal range to be from 4.5 to 6.5 cc per liter at temperatures of 15° to 20° C.

A careful consideration of the data presented by Powers and Shipe in support of their contention fails to reveal any adequate indication of a direct relationship between O_2 tension and O_2 consumption in the restricted range we are considering. The fact that their experimental conditions were not standard may account for the fact that a number of their recorded values for oxygen consumption (cc per kilo per hour) are several times those previously reported for fishes. As for Hall's evidence, it is to be regretted that it is inadmissible on the grounds of faulty technique. Hall used a modified Ege and Krogh[3] apparatus in which the oxygen tension of the water was controlled by varying the rate of flow; in other words, the concentration of oxygen was diminished by the respiration of the fish and was then rebreathed in the large respiratory chamber used. It is obvious that the fish is excreting carbon dioxide at approximately the same rate that it is using oxygen and, even in a buffered system such as sea water, this will displace the hydrogen-hydroxyl-ion equilibrium and lower the pH. The marked effect of carbon dioxide on respiration is too well known to require comment. In addition, the accumulation of nitrogenous excretions in the water may possibly exert some effect.[4]

Hall mentions the work of Winterstein[5] in which it was concluded that the oxygen consumption of the fish (*Leuciscus*) is independent of the oxygen tension over a wide range. The important papers of Henze,[6] Gaarder[7] and Toryu[8] are apparently unknown to Hall and to Powers and Shipe. Henze concluded that the oxygen consumption of fishes is entirely independent of the oxygen of the water down to the threshold of asphyxia. Toryu drew similar conclusions from his experiments on the goldfish. Gaarder, using precise methods, found some indication of a direct relation between the oxygen tension in the water and the oxygen used by the fish, but concluded that, within a limited range, the oxygen consumption may be considered to be practically constant.

In my own experiments, water of really low oxygen tension was not used; the minimum concentration was 4.56 cc per liter. My methods were similar to those of Gaarder, but I did not narcotize the fishes as he did. Experiments on the same individual fishes (*Fundulus parvipinnis*), under identical conditions except for the oxygen tension of the water, did not give the slightest indication that the oxygen content of the water has any effect upon the respiration, but on the contrary the oxygen consumption was practically constant for any given fish. Experiments in which both the rate of flow and the oxygen concentration of the water were varied were likewise entirely negative, the respiration of the fish, based on the averages of a number of determinations, remaining remarkably constant, such deviations as were found having no relation to the available oxygen.

After considering all the available evidence it seems clear that the time has not yet come when we can make any very broad generalizations as to the relation of the oxygen tension of the environment to the oxygen consumed by the fish, but certain features are salient: (1) the respiration of fishes is very decidedly decreased at or near the asphyxial level of oxygen tension, and (2) in the respiration of the fishes so far investigated there seems to be practically a complete independence of deviations in the oxygen content of the water within a restricted range.

The practical importance of establishing this last point will be seen when it is pointed out that the methods which have previously been used for the estimation of the standard metabolism of fishes depend upon such an independence of the fish's respiration for their validity.

A more detailed discussion and presentation of evidence will shortly appear in the technical series of the bulletins of the Scripps Institution of Oceanography, University of California Press.

ANCEL B. KEYS
SCRIPPS INSTITUTION OF OCEANOGRAPHY

[3] R. Ege and A. Krogh, *Internat. Rev. ges. Hydrobiol. Hydrog.*, 7(1): 48–55, 1914.
[4] H. W. Smith, *J. Biol. Chem.*, 81(3): 727–742, 1929.
[5] H. Winterstein, *Pflüger's Arch. ges. Physiol.*, 125: 73–98, 1908.
[6] M. Henze, *Biochem. Zeitsch.*, 25: 255–278, 1910.
[7] T. Gaarder, *Biochem. Zeitschr.*, 89: 94–125, 1918.
[8] Y. Toryu, *Sci. Rept. Tohoku Imp. Univ.*, 4 ser. 3 (1): 87–96, 1927.

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HAMBLY, WILFRID D. *Ethnology of Africa.* Pp. 226. 42 plates. Field Museum of Natural History.

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